

Alma Mater Studiorum – Università di Bologna

**DOTTORATO DI RICERCA IN
ECONOMICS**

Ciclo 31

Settore Concorsuale: 13/A1

Settore Scientifico Disciplinare: SECS-P/01

ESSAYS ON POLICY EVALUATION

Presentata da: ROBSON DOUGLAS TIGRE SANTOS

Coordinatore Dottorato

Supervisore

Prof. Marco Casari

Prof. Paolo Masella

Esame finale anno 2019

Acknowledgments

First I thank the members of this department, who supported me throughout this journey and, in their own ways, taught me some lessons for life. To cite only a few, Maria Bigoni, Marco Casari, Matteo Cervellati, Vincenzo Denicolo, Silvia Fiorentini, Paola Mandelli, Paolo Masella, my supervisor, Chiara Monfardini, and Giovanni Prarolo. I am specially grateful to the friends I made from the day we started here, and who were there on daily basis supporting one another with our motto (you guys know what it is!). I also thank Giovanni Mastrobuoni and Lorenzo Rocco for the suggestions on how to improve the chapters. At last, I acknowledge the PhD and Marco Polo fundings provided by the University of Bologna and the Italian Ministry of Education, Universities and Research.

“The disciple is learning when he does not know that he is learning, and as a result he may well chafe. In winter, (...) a tree is collecting nutrient. People may think that it is idle, because they do not see anything happening. But in spring they see the buds. Now, they think, it is working. There is a time for collecting, and a time for releasing. This brings the subject back to the teaching: ‘Enlightenment must come little by little – otherwise it would overwhelm’.”

Idries Shah - The Sufis, 1964.

Abstract

This document is a collection of three articles developed during the course of my PhD, which I submit as requirement for the final exam. The three papers focus to some extent on policy evaluation, applying two main microeconomic techniques: regression discontinuity design and difference-in-differences. Each paper is briefly described as follows.

Daylight Saving Lives In this paper I investigate the effect of Daylight Saving Time (DST) on homicides. As assignment into the policy follows technical reports from the National Electric System Operator, the Brazilian setup lends itself to both within and between-states comparison of homicide levels around the date of transition and during the whole period of DST adoption. Using a difference-in-differences strategy, I find a decrease in the number of homicides by firearms of roughly 9.83% during DST months. In line with the crime deterrence hypothesis, I show this uncovered effect is mostly concentrated in hours directly affected by the shift in daylight caused by DST - i.e., during early evening hours. Back-of-the-envelope calculations suggest the shift in clocks was responsible for saving about 5,035 potential victims from 2006-2015.

Educated Candidates and Efficient Bureaucrats I test whether more educated candidates make into less corrupt public managers. Leveraging on a randomized inspection program and close-race elections, I show that college-degree candidates commit 8-11% less infringements than their less educated peers, which is driven by a lower level of moderate – instead of formal or severe - non-compliance. That higher bureaucratic compliance, however, is not correlated with decreased diversion of public funds. Nevertheless, wrongdoing by less educated candidates translate into lesser discretionary transfers granted in the subsequent mandate, which may result in diminished public goods provision. Moreover, after ruling out electoral-concern mechanisms, I show that college-degree candidates spend relatively less with administration, which is driven by an inflation in temporary staff 36.8% smaller. Taken together, results suggest that differences in widely used measures of corruption do not actually stem from corrupt behavior being correlated with politicians' education, but from differences in bureaucratic efficiency.

Accountability Shock and Market for Oversight In this paper I tests whether a top-down shock in oversight and political accountability can strengthen the market for transparency in public management. As the source of accountability saliency, I take advantage of an anti-corruption program that selects municipalities for inspection of public funds through publicly held lotteries, causing exposition of mismanagement and corruption to be independent of potential levels of malfeasance, pre-existent level of citizens' engagement, and pre-existent efficiency of political and legal institutions at the local level. Results show that disclosure of information on misuse of public funds, on average, increases citizens' demand for information on public affairs; boosts citizens' interaction with the Federal ombudsman, also increasing the number of denouncements and complaints against public officials and institutions; increases the number of lawsuits filled against public agents both by public prosecutors and by ordinary citizens; and promotes compliance with transparency guidelines by the exposed institutions.

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FIRST CHAPTER
DAYLIGHT SAVING LIVES

1.1 Introduction

In this paper we investigate the effect of daylight saving time (DST) on homicides. For that, we use hourly data on mortality over a period of ten years and variation in DST adoption across states to test if the exogenous shock to ambient light caused by the policy led to a decrease in homicides.

For the purpose of this exercise we analyze one of the most violent countries in the world – Brazil. In 2015, the country led the ranking in homicide counts with 55,574 deaths, way above the (more populous) second place, India, which recorded 41,623 intentional homicides. In that same year, the homicide rate in the country was 267.4 per 1,000,000 inhabitants, approximately five times the rate observed in the United States.¹ Like many developing countries, much of this violence is associated with drugs and wide availability of guns in the black market (Reichenheim et al., 2011), and matching this profile homicide represents the leading cause of death for men aged 15-44 in Brazil, with 90% of the cases involving firearms (De Souza et al., 2007).

Those alarming numbers are in contrast with the country's gun-control efforts. In the beginning of the 2000s the Government passed extensive laws aimed at controlling the flow of firearms into the country, instituting strict background checks for gun purchases and registration, and making it illegal for civilians to conceal even registered guns outside their home or business. This is alleged to have saved more than 2,000 lives in a four-year period in the state of São Paulo alone (Cerqueira and de Mello, 2013)². However, these measures were not sufficient to impose a substantial change in the dynamics of homicides in the country. In 2014 the number of registered intentional homicides was 52,336, 3.8% larger than the number registered in the previous year.

In this context, we show that a policy consequence as simple as a shift in ambient light during early evening hours caused by DST can exert an impact on the relative cost of committing a homicide in such a way that would-be offenders choose not to do so.

¹Retrieved from United Nations Office on Drugs and Crime - UNODC Statistics (<https://data.unodc.org/> - Last accessed on September 6 2018).

²See also the recent work by Schneider (2018).

We follow the rationale of deterrence in crime-supply, a model in which individuals' likelihood to engage in crime decreases as the probability of getting caught and the severity of punishment increase. In this framework, luminosity during otherwise high-homicide hours facilitate witnesses and law enforcement agents to detect perpetrators, thus increasing perpetrators' expected cost (Doleac and Sanders, 2015).³

To do so, we exploit the exogenous intra-day shift in light period caused by Daylight Saving Time to estimate the effect of light-time in otherwise dark hours on homicide occurrences. The quasi-experiment induced by Daylight Saving Time can serve as an alternative potentially as good as randomization to identify the effect of ambient light on homicides, and hence the effect of the DST policy, as the source of variation is completely exogenous to criminal decision-makers and to states adopting the policy, as will be further discussed.

Although recent empirical literature has already exploited DST to identify causal effects on a broad range of outcomes, including criminal activity (Doleac and Sanders, 2015), to the best of our knowledge the present paper is the first to find evidence of impact on homicide occurrence.⁴ This is especially important for a middle-income country like Brazil, where the burden of state absence falls disproportionately on those less well-off. If we consider intensity of night light as a proxy for public outdoors illumination, while Brazil has a share of urban population 80% greater than that of Guatemala, and a GDP per capita roughly twice as great, the former is less lit than the latter throughout the whole distribution of night time illumination (Henderson, Storeygard and Weil, 2012). Our findings thus may suggest that policies as simple as providing adequate public illumination can significantly deter lethal violence.

On a more general perspective, we provide important input into the debate concerning the cost-effectiveness of DST. There is a constitutional amendment process

³This may seem controversial at first, since someone's likelihood of interacting with a criminal may increase if he/she stays out later than he/she otherwise would, thus increasing the "demand" for crime even as we expect the "supply" for crime to decrease. However, evidence using sophisticated technology on gunshot detection show that removing bystanders and potential witnesses from public areas actually increases gunshot incidents (Carr and Doleac, 2018).

⁴Other examples are Kountouris and Remoundou (2014), who analyze the impact of DST on individual well-being, Smith (2016), who studies the impact of this variation on fatal vehicle crashes, and Toro, Tigre and Sampaio (2015) which investigate the effect of sleep disturbances on myocardial infarction.

currently with Cabinet to extinguish the daylight saving time in Brazil.⁵ The argument behind the amendment being that DST may have adverse effects on the health of the population while it brings no benefit to the economy in terms of energy savings. Along the same line, the European Parliament has recently voted (February/2018) in favor of reviewing the bi-annual change of clocks across the bloc after a citizens' petition, which gathered 70,000 signatures, urging Brussels to abolish the centralized switch in time due to health concerns. Our numbers, however, show that positive health externalities from DST might have been largely overlooked.

Given the tremendous role firearms play in homicides in Brazil, we focus on deaths for which firearm discharge was the cause, using data from the Information System on Mortality (SIM) implemented by the Brazilian Ministry of Health. To provide a statistically well-founded and accurate estimate of the effect of DST on homicides we use a well defined control group, namely Brazilian states that by law are not affected by the policy, in a difference-in-differences framework. This allows us not only to estimate the effect of DST on the whole period during which the policy is adopted but also to control for confounding factors. In general, we find that homicides by firearms decreased during DST months by about 9.83%.

A constant concern in empirical studies is the possibility of the treatment being correlated with unobservable factors, leading to a spuriously estimated effect. In our framework, this means that the timing of DST adoption may coincide with an event neglected by the analysis that is the actual driver of homicide deterrence, though through channels other than ambient light. In that regard, we conduct some additional tests to check the mechanism and the robustness of our results. First, we investigate whether results are consistent with our theoretical predictions that suggest a strong decrease in criminal behavior exactly in the hours most affected by DST. Accordingly, we find compelling evidence that the effect we estimate is mostly concentrated in the hours around sunset, which observe a 28.82% decrease during DST period. Second, we test for the existence of anticipatory effects by estimating the models allowing for

⁵Senate bill 438, 2017 - Senator Aírton Sandoval (<https://www25.senado.leg.br/web/atividade/materias/-/materia/131542> Last accessed on April 6 2018).

leads and lags, along the lines of Autor (2003). We find that anticipatory effects are virtually zero whereas lagged effects are substantial, adding credibility to the common trend assumption.

Third, we draw from Dell (2010) and Lalive et al. (2014) and design a more convincing comparison between treated and control municipalities by truncating the sample to observations falling within a certain distance from the border that divides both groups of states (DST adopters and non-adopters), making comparisons more balanced in terms of economic and geographical features. We find strong evidence that homicides decreased during DST and that this reduction is largely driven by declines in homicides around sunset. Fourth, along the lines of Doleac and Sanders (2015) and Smith (2016), we exploit a three-week variation in the start of DST to estimate the effect of the policy on dates that are under Standard Time in 2006 but under DST in other years within adopter states. Again, we find significant effects of DST on homicides that are concentrated on the hours around sunset. Fifth, we investigate the transition back from DST to Standard time to check whether the light mechanism works in favor of increasing homicides in hours directly affected by the shift. We find positive and significant increases in homicides on hours around sunset.

As a final and complementary strategy, we use local-polynomial regression discontinuity estimators with bias-corrected non-parametric confidence intervals (Calónico, Cattaneo and Titiunik, 2014) to estimate the short-term effect of the policy on homicide deterrence around the transition date. Although we are not able to obtain the overall effects of the DST policy, this approach provides internal validity as comparisons are made using a subset of observations restricted to a shorter time span around the transition date, making estimates less susceptible to unobservables, as other factors that affect homicides should evolve smoothly over the year. Accordingly, we estimate the effect of DST separately for states that adopt and that do not adopt DST, using daily and hourly data, and document significant decreases in homicides that are largely concentrated on hours directly affected by the shift on treated states and statistically insignificant changes on states that are exempt from DST adoption, confirming our

hypothesis.

Taken together, these estimates imply Daylight Saving Time is responsible for saving about 5,035 potential victims from 2006-2015.⁶ This number is more than 35% above the total number of homicides that occurred in 2013 in the nineteen countries located in Northern and Western Europe. Building on the value of statistical life in Kniesner et al. (2012), which ranges from \$4 to \$10 million, we estimate that DST resulted in 2.01-5.03 billions in annual social cost savings from avoided homicides. In addition to this direct effect, a significant reduction in homicides could also have large long-lasting indirect effects. For instance, recent estimates provided by Koppensteiner and Manacorda (2016) using data from Brazil show that exposure to a homicide during the first trimester of pregnancy considerably reduces gestational length and birth-weight. This further reinforces the importance of our empirical findings.

The remainder of the article is organized as follows. Section 1.2 describes structure and institutional framework of DST in Brazil, while sections 1.3 and 1.4 present the data set and empirical strategies we exploit. Finally, section 1.5 discusses the results, and conclusions are presented in section 1.6.

1.2 Daylight Saving Time in Brazil

DST is a policy designed to save energy and is currently adopted by more than 70 countries, affecting more than 1.5 billion people yearly. It takes advantage of variation in the distribution of sunlight time between seasons to reallocate ambient light to the evenings, which is done by shifting the relationship between clock time and sunset by (usually) one hour.⁷ In Brazil, DST has been adopted every year since 1986, although its current form was defined only in the 2000s.

Historically, DST in Brazil has been governed by Federal enactments, usually based on information from technical reports provided by The Electric System National Op-

⁶This figure is based in the total number of homicides that occurred during the three months of DST for treated states in the course of 10 years (2006-2015), which amounts to 46,345 homicides. Our main estimate suggests a reduction of 9.83%. This leads to a reduction of about 5,035 homicides throughout the period analyzed — i.e., $[46,345/(1 - 0.098)] - 46,345 = 5,035$.

⁷A review of the origins of DST and its early adoptions is presented in Aries and Newsham (2008).

erator (ONS). Aiming at maximizing potential energy saving, the National Operator suggests to the Federal Government which states should adopt DST and the duration of the regime.⁸ Given the main motivation for the existence of this policy is the variation in sunlight induced by the summer solstice in the Southern Hemisphere, DST implementation does not provide significant benefits for states closer to the Equator line. This leads to variation in the treatment status across the country. Its technical basis, provided by the ONS, jointly with the compliance enforced by Federal legislation, favor our identification strategy since it provides variation in DST adoption both between (i.e., adopters vs. non-adopters) and within states (i.e., among those that adopt; standard time vs. DST).

Throughout the entire time span we analyze (2006-2015), all Brazilian states within Midwestern, Southern and Southeastern administrative regions, where light incidence vary the most across seasons, adopted DST, while no states in Northern and Northeastern administrative regions did, except for Bahia (BA) and Tocantins (TO), which adopted DST in 2011 and 2012, respectively, for political reasons (see Figure 1).⁹ Having non-adopter states helps us in designing additional placebo tests, given other factors affecting homicides besides DST are expected to evolve smoothly around the transition date in states that did not adopt the policy.

[Figure 1 about here.]

DST in Brazil usually starts on the third Sunday of each October, when clocks skip forward from 12am to 1am, and extends until midnight of the third Sunday of each February, when clocks fall back one hour to standard time. A detailed list of adopters by year is provided in Table 1 below. A particular and interesting change in the starting

⁸We remark “maximizing *potential* energy saving” as there are controversies over whether DST policy in fact saves energy (Havranek, Herman and Irsova, 2018). For the 2017-2018 DST term, the Brazilian Electric System National Operator (ONS) obtained statistically insignificant estimates of the impact this policy had on energy consumption in the adopter states - https://agentes.ons.org.br/download/avaliacao_condicao/horario_verao/RE0050-2018_AvaliacaoHV2017-2018.pdf (Last access on February 6 2019).

⁹Bahia and Tocantins, which are contiguous to adopter states, exceptionally joined a DST term each. Due to their geographic position, however, DST had minor relevance in electricity saving (according to the Government), and following immense popular dissatisfaction, state governors asked to be dismissed in the following years. In any case, results are qualitatively the same regardless the inclusion of those state in our estimations.

date of DST occurred in 2006, when the policy started three weeks later than normal, on November 5. This provides us an additional opportunity to directly control for time-of-year effects among adopter states, along the lines of Doleac and Sanders (2015) and Smith (2016), who considered law changes to DST policy in the U.S. to account for endogeneity, since DST occurs simultaneously across 48 states (Arizona and Hawaii do not observe DST) and at approximately the same time each year.

[Table 1 about here.]

1.3 Data and descriptive statistics

1.3.1 Data source

We use data on homicides for the period of 2006-2015 retrieved from the *Sistema de Informações sobre Mortalidade* (SIM), the national information system on mortality, implemented by the Brazilian Ministry of Health. This information system was designed to provide daily information on mortality to local and federal authorities, claiming global coverage within national borders. To this end SIM relies on legal certificates of death as its data input, which are strictly regulated by the Federal Government.¹⁰

Official declarations of death contain two features that provide means to support our claims, namely time and cause of death according to the International Classification of Diseases, ICD-10. Among the “environmental events and circumstances” listed in ICD-10, we focus on deaths caused by firearm discharge, as they play an important role in criminal interactions in Brazil (De Souza et al., 2007; Reichenheim et al., 2011). Since there is serious evidence of a large share of intentional homicides being misclassified as fatal incidents of undetermined intent (Cerqueira, 2012, 2013), we consider both deaths due to assault and with undetermined intent in this study.¹¹ The complete list of ICD-10 codes we use to construct our dependent variable is provided in Table 2.

¹⁰Published in October 9th of 2003, Federal Ordinance MS/SVS nº 20 provides a rulebook on the filling out of declaration of death forms, which are standardized and distributed by the Ministry of Health.

¹¹In fact, evidence regarding systematic misclassification comes from the Institute for Applied Economic Research (IPEA), a federal public institution directly linked to the Secretariat of Strategic Affairs of the Presidency (SAE/PR).

It is important to notice that fatal incidents of undetermined intent do not include accidental deaths caused by firearms, which are classified under the ICD-10 codes W32-W34. Additionally, we opt to exclude from our estimations homicides resulting from legal intervention, since according to ICD's methodology those are the result of law-enforcement agents on duty in the course of arresting or attempting to arrest lawbreakers, and therefore would mistakenly favor our hypothesis, as deaths from legal intervention are expected to be positively correlated with lethal criminal activity.

[Table 2 about here.]

1.3.2 Descriptive statistics

In table 3 we present unconditional averages of homicide rates for one week prior and one week after DST transition, disaggregated by treatment group. Columns 1-2 present results at the daily level, whereas columns 4-5 present it for the sunset hours (i.e. relative hours 0 and 1, with reference to sunset time).¹²

We note that for municipalities that adopted the policy, the average homicide rate is 0.467 on the week prior to transition to DST. On the week following transition, it decreases to 0.437, a reduction of almost 6.5%, statistically significant. If we consider only homicides occurring around sunset hours, this decrease amounts to about 20%. These patterns are not observed for those municipalities that did not adopt the policy: A statistically insignificant reduction of 1.6% is observed when considering daily level, and of 1.98% when looking at hours around sunset.¹³

[Table 3 about here.]

For further transparency, we plot in figure 2 the evolution of homicides along several weeks across the transition date. We plot polynomial splines with no controls

¹²We standardize clock-time with respect to sunset-time. This means that relative hour 0 is the hour that starts at the exact sunset hour.

¹³See table A1 in the Appendix for social-economic information on deceased individuals as well as municipality characteristics for treated and control municipalities. For instance, we observe that treated States have greater per capita GDP, larger share of the population living in urban areas, and larger population density, hence the importance of municipality fixed effects (and trends) in our main specification.

for each side of the transition to distinguish trends from seasonality caused by an increase in homicides during weekends. Given the fairly similar pre-treatment trend between treatment groups, it is reasonable to assume that, in the absence of DST, adopters (i.e. dark gray lines) would show an ascending trajectory in homicides similar to that observed for control municipalities (i.e. light gray lines). The former group, nevertheless, exhibits a minute descent followed by a flat trajectory.¹⁴

[Figure 2 about here.]

As a last descriptive exercise, and to support the hypothesized mechanism of ambient light as a deterrence tool, we plot in figure 3 the distribution of homicides by hour of the day during DST and standard time. For each treatment group, gray bars represent the density of homicides in a given hour during the DST period, whereas white bars refer to the standard time period. Panel (A) shows that most of the variation across transition to DST occurred during sunset hours - vertical dashed lines, which represent the minimum, mean, and maximum sunset hours of municipalities during standard-time. On the other hand, panel (B) shows minute differences, if any, in the distribution of homicides for any given hour across transition to DST.

[Figure 3 about here.]

Despite the suggestive results from the descriptive analysis carried out so far, from now we discuss and apply formal methods to defend the hypothesis that the observed effect is in fact caused by the policy in question.

1.4 Research design

In this section we present the empirical strategy used to identify the causal effect of the daylight saving time on homicides. We consider first using variation between and within states in a difference-in-differences framework to estimate the overall impact

¹⁴Note that the upward shift just after transition for control States is a virtue of the adjusted polynomial. We show below that homicides evolve smoothly around the transition date for States without DST.

of the policy. Second, to test for the mechanisms behind the effect of the policy on homicides invoking weaker assumptions, we estimate the short-term effect of the policy using a regression discontinuity design (RDD) which does not rely on the common-trend assumption.

1.4.1 Overall effect of the policy

To estimate the effect of the daylight saving time for the whole period of the policy, we consider first estimating the following fixed effects model,

$$(1) \quad H_{icy} = \beta_0 + \beta_1 DST_{icy} + WeekDay_i + \lambda_i + \lambda_c + \lambda_y + \varepsilon_{icy}$$

where H_{icy} is the homicides rate in day i , municipality c and year y . DST_{icy} is an indicator variable that assumes value equal to one for municipalities that adopt the policy as from the date of transition and zero otherwise, and λ_i , λ_c , and λ_y represent, respectively, day of year, municipality and year fixed effects. The day of year fixed effects flexibly control for seasonality of homicides for each day of the year. For instance, we might observe more homicides occurring in the first day of each month, due to employee's payday (Evans and Moore, 2012).¹⁵ Municipality and year fixed effects control non-parametrically for municipalities fixed differences and year shocks common to all municipalities, respectively. Finally, $WeekDay_i$ controls for persistent day-of-week effects, since homicides might more frequently occur during weekends than on weekdays (see the dynamics presented in figure 2).

In this simple model, our parameter of interest, β_1 , measures the average effect of the policy during the whole period of DST. It amounts to calculating the difference in homicides between that observed during the policy and that observed during standard time for states that adopt and states that do not adopt DST.

Accordingly, we estimate the model first using daily data to assess the overall

¹⁵Here we follow Smith (2016) and create dummies for each month/day combination (for instance, we create an October 14th dummy). This differs from creating a dummy for each day of the year, because leap days would cause October 14th for most years to be matched with October 13th for some years.

effect of the daylight saving time. Since the social cost of crime is high, especially for homicides, any movements in criminal activity due to shifts in daylight during early evening hours caused by the policy could add much to the current debate on the cost-effectiveness of the DST. Nevertheless, the number of daily homicides need not respond to the transition if criminals relocate their “working” activities from hours directly affected by the shift in daylight to other hours of the day. For instance, criminals might increase activity to hours which are dark both directly before and after DST. If they indeed adjust their labor supply, the DST might cause no effect on aggregate homicides even though light could still affect criminal behavior. To investigate this possibility, we estimate the effect of interest for different hours of the day. Following the crime deterrence rationale, we expect to see a strong decrease in homicides on hours of the day that are directly affected by the transition, i.e., hours that were dark before the transition date and turned out to be light after transition due to the shift in clock time.¹⁶

In addition to the basic difference-in-differences estimator presented in equation 1, we employ a flexible event-study specification to investigate in more detail the evolution of homicides in municipalities adopting and not adopting DST policy (Jacobson, LaLonde and Sullivan, 1993; Goodman-Bacon, 2018). To do so, we consider estimating the model with additional dummies indicating weeks before and after adoption of the policy to check whether causes happen before consequences (Angrist and Pischke, 2014), along the lines of Granger (1969). We estimate the following specification

$$(2) \quad H_{icy} = \beta_0 + \sum_{\tau=-21}^{10} \beta_{\tau,1} DST_{\tau,icy} + WeekDay_i + \lambda_i + \lambda_c + \lambda_y + \varepsilon_{icy}$$

where $DST_{\tau<0,icy}$ is set to one for a given week if municipality c will adopt DST τ weeks in the future, and zero otherwise. Likewise, $DST_{\tau>0,icy}$ is set to one for τ weeks after DST transition, and zero otherwise. We consider 21 leads or anticipatory effects

¹⁶Accordingly, we might also observe the opposite during hours that were light before the policy and turned out to be dark after DST. We find no such effects. We note however that most crimes occur during the evenings (see figure 3). Additional ambient light during mornings seem to provide no additional incentive to lazy criminals, especially following a one-hour-short night of sleep.

and 10 lags or post-treatment effects, in the fashion of Autor (2003).

On one hand, this allows us to check the robustness of our results to changes that might occur close to the implementation of the policy, as our primary identifying assumption is that in absence of the policy, municipalities adopting DST would have experienced similar trends in homicides as municipalities not adopting the policy. If municipalities adopting the policy have pre-treatment trends that differ substantially from that observed in municipalities that do not adopt, then our parameter of interest might mistakenly attribute pre-existing trends in homicides to our treatment effect. On the other hand, by allowing the model to have lagged effects, we can also measure whether the impacts of the policy are sustained over time or whether they merely reflect a short-run reaction to the DST transition. For instance, in addition to the ambient light mechanism which should permanently affect homicides during directly affected hours, the short-term disruption in sleeping patterns caused by the standard to DST shift may further decrease homicides in the first or second week after transition (Munyo, 2018). This sleep mechanism may affect other hours of the day that remain either light or dark after transition, via reductions in criminal activity.¹⁷

1.4.2 Effect around the transition date

To test for the existence of these short-run effects, we complement our analysis by using a regression-discontinuity design to estimate the short-term effect of the transition from standard to daylight saving time on homicides. The advantage of this strategy is that we need not rely on the common-trend assumption, as comparisons are made within treated states. The drawback is that we obtain only local average treatment effects.

We compare homicides before entering DST to homicides after the transition for states that adopt DST using an optimally-chosen time interval around the transition

¹⁷Although there has been surprisingly little empirical evidence on the effects of sleep on worker productivity, Gibson and Shrader (2014) has shown that a one-hour increase in long-run average sleep increases wages by 16%.

date. For that, we consider the following reduced-form model

$$(3) \quad H_{icy} = \tau I(T_{icy} \geq 0) + g(T_{icy}) + \varepsilon_{icy}$$

where H_{icy} is defined as above, T_{icy} is the running variable, defined as the number of days to/from transition to DST, which is equal to zero on the first day after transition and positive after (negative before), g is a (local linear regression) function of that variable, and ε is the error term.

We use local-polynomial point estimators and recent robust bias-corrected confidence intervals (Calonico, Cattaneo and Titiunik, 2014) to estimate τ . In this framework bandwidth selection is crucial since it imposes a trade-off between bias and variance. Therefore, in contrast to previous literature in this field, we rely on optimal data-driven bandwidth selectors to set the time interval considered for comparison around the transition date. Specifically, we show results using the bandwidth selector outlined in Calonico, Cattaneo and Titiunik (2014), hereafter CCT.¹⁸

As we mentioned above, consistently estimating the short-run effect of interest requires the outcome to evolve smoothly around the transition date in the absence of treatment (once we control for day-of-week and other fixed effects), an assumption that cannot be directly tested due to the observational nature of our analysis. In contrast with previous papers, however, and due to our institutional setup, we can use untreated states to test whether unobserved factors at the national level are spuriously correlated with the transition date. Hence, we estimate similar models considering the sample of states that do not adopt the policy.

¹⁸As discussed in Calonico, Cattaneo and Titiunik (2014), previous bandwidth selectors tend to yield large bandwidths, leading to biased confidence intervals. We adopt CCT as our main procedure, but results are equivalent the same when using Imbens and Kalyanaraman (2012) procedure. Estimates using ad hoc bandwidths are available upon request and are quantitatively the same. Additionally, for this analysis, we aggregate the data at the state level, since the CCT selection procedure failed to find an optimal bandwidth using hourly county-level data due to the large amounts of zeros observed around the discontinuity.

1.5 Results

1.5.1 Main Results

We begin by examining the overall effect of the DST policy on homicides. In panel A of table 4 we present difference-in-differences estimates of the effect of the policy on daily homicides. We consider four bandwidths around the transition date, presented in columns 1-4. We find that the homicide rate decreases by .025-.028 throughout DST period. The consistency of the estimates using different bandwidths adds credibility to our empirical framework, as results are apparently stable across the treatment period. Relative to the mean of .293 observed for treated municipalities on the period before treatment, this represents a decrease of around 9.83% in homicides during the three month period of DST adoption. Note that besides including a considerable amount of controls, municipality-specific time trends marginally changes the parameter of interest, as presented in the estimates of column 5 compared to column 4, adding robustness to our causal claim.

[Table 4 about here.]

In panel B we estimate the same model, but considering only crimes that occurred around sunset. As discussed above, the main mechanism behind potential reductions in homicides following DST transition is the shift in ambient light that affects specific hours of the day. Following this rationale, we expect crime to decrease most in hours directly affected by the transition: those in the periods covering the hour of sunset and that directly following sunset (dusk), i.e., relative hours 0 and 1, with reference to sunset time.¹⁹ Results show that the homicide rate in these hours decrease by .009-.013, which amounts to a 28.82% decline in homicides, relative to the mean of .0322 observed on pre-treatment period. We emphasize that in these specific hours we observe an abnormal concentration of homicides when compared to other light

¹⁹ Accordingly, hours before (after) sunset include the two hours preceding (following) the sunset (dusk) hour.

hours of the day, as evidenced in Figure 3, making this impact quantitatively relevant. Again, we highlight that our estimates are quite similar when including trends for each municipality.

1.5.1.1 Event studies

The results presented above suggest that the transition to DST is followed by a significant decrease in homicides. However, it says nothing about whether these effects are sustained over time nor does it say anything about whether there were any changes in homicides on treated municipalities that might coincide with the transition to DST. If, for instance, we fail to find support for the identifying assumption that there exists no major differences in homicide trends between treated and control municipalities before the shift in daylight, then the estimates presented above are likely to be subject to omitted variable bias. Along the same lines, if reductions in homicides are mostly driven by the shift in ambient light during early evening hours, we expect to observe significant effects throughout the whole DST period.

In figure 4 we plot the estimated difference-in-differences coefficients on event time using equation 2, along with the respective 95 percent confidence intervals. In panel A we observe the dynamics of pre-treatment and post-treatment effects for daily homicides, while in panel B we observe this dynamics for sunset hours only. The small and statistically insignificant coefficients obtained for the 21 weeks before DST transition support the identifying assumption that municipalities without DST would have had similar trends in homicides in the absence of the policy. This strongly suggests that the treatment and control groups are comparable in terms homicides trajectory and that criminals do not anticipate the shift in ambient light, making DST transition likely to be exogenous.

[Figure 4 about here.]

On the contrary, the estimated effects in the weeks following the transition are substantial for both daily and hourly (around sunset) homicides. We observe a sharp and immediate decrease in crime following DST transition on both graphs. More

importantly, these effects do not fade away as we move into weeks following the transition date. Although we observe some variation in the magnitude of the effect, they are qualitative similar and significantly different from zero.

1.5.1.2 Time allocation

We hypothesize that DST should have the strongest effect during the hours of light transition, i.e., those around sunrise and sunset. This, however, need not imply that criminal activity during other hours of the day that remain either dark or light would not respond to the policy. For instance, criminals could shift labor activities to the remaining dark hours of the day, or could eventually increase activities during light hours, as to achieve some predetermined level of income derived from crime. To investigate this, we extend the results presented above to two-hour periods throughout the whole day around the standardized sunset hour.

[Figure 5 about here.]

Figure 5 plots difference-in-differences estimates for each hour-group around sunset throughout the whole DST period. We observe that the largest and statistically significant impact occurs exactly on the hours directly affected by shift (sunset). Although we observe some estimates to be negative and statistically significant for other hours of the day, which in part suggests that criminals are not reallocating their activities, they are quantitatively small in size. For instance, the estimates for 6 and 12 hours before sunset are three times smaller than that obtained for the sunset hours.²⁰ Taken together, we provide suggestive evidence that homicides decrease after DST transition and that this effect is mostly explained by the light mechanism.

²⁰Note that these estimates need not represent those for the hours around sunrise. For instance, twelve hours before sunset may coincide with sunrise hours for some municipalities located in the Northeast of Brazil but it may not coincide for other municipalities located in the South of the country. We estimate the effects of the policy on sunrise hours and find small and statistically insignificant effects. Results are available upon request.

1.5.1.2.1 Geographic balance and time span

A potential concern with our results so far regards the fact that the duration of light-hours throughout the day changes for municipalities located in different hemispheres as we move along the year. For instance, for municipalities located in the Northeast of Brazil, closer to the equator, the number of light hours increases as we approach the summer but by less than the increase observed in municipalities located in the southern regions. As the number of light hours increase more in municipalities located in states that adopt DST (see figure 1), we might observe a smooth and relatively larger decrease in homicides on these municipalities when compared to municipalities located in states that do not adopt DST. Results presented in figure 4 suggest that, if any, the smooth increase in the number of light hours in DST-adopting states is likely to be of small importance, as we fail to observe changes in the dynamics of homicides between treatment and controls municipalities before the policy was enacted, and the estimates afterwards appears not to increase monotonically with time.

To account for this potential concern, however, we draw from Dell (2010) and Lalive et al. (2014), and design a more conservative comparison between treated and non-treated municipalities by looking at regions balanced in terms of geographical features. Besides differences in the dynamics of light-hours throughout the year, geography has played an important role to economic development in Brazil. In the late 19th and early 20th century, for instance, agricultural motivated a state-sponsored policy that attracted European immigrants with higher levels of human capital to settle in some specific regions within our treated area (Rocha, Ferraz and Soares, 2017). Comparatively, such events are reflected in local socioeconomic characteristics such as lower illiteracy rates, improved educational opportunities and, conceivably, lower incentives to engage in criminal activity.

Therefore, the idea behind the proposed exercise is to truncate the sample to observations falling within a certain distance from the border that divides the group of treated from the group of untreated municipalities. That is, for each side of the border that separates the Midwestern, Southeastern, and Southern regions (which

contains states that adopt DST policy) from the Northern and Northeastern regions (containing states that do not adopt DST), we run regressions restricted to observations that fall within a given distance of that border.²¹ By focusing on observations that are closer to the border, we also avoid possible outlying values from large urbanized areas such as state capitals, which are all located further from this imaginary line for most of the distances tested. Of course, this comes at the cost of reduced statistical power.

In figure 6 we present difference-in-differences estimates for each group of hours of the day while restricting the sample used in figure 5 to municipalities within a band of 190km (10th percentile of the distance-to-border distribution) from each side of the border that divides treatment and control groups. Note that standard deviations are considerably larger due to a significant reduction in sample size. Accordingly, we find a large and statistically significant reduction in homicides on hours around sunset. Estimates for all other hours are virtually zero and not statistically significant. Relative to the mean of .0602 observed on pre-treatment period, a reduction of -.0156 observed for the sunset hours imply a 25.8% decrease in the homicide rate.

[Figure 6 about here.]

Following the same logic as above, estimates presented in figure 5 are calculated using observations for the whole DST period. Accordingly, since the duration of DST is of around 3 months, we present in figure 7 estimates in which we restrict the bandwidth around the DST entry date to 30 days. This should make hour comparisons less susceptible to changes in the dynamics of homicides that might arise from differences in the duration of daylight throughout the year. Following what we consistently report above, we find a large (and similar in magnitude) decrease in homicides on the hours directly affected by the transition.²²

[Figure 7 about here.]

²¹See figure 1. The aforementioned border is the one dividing states in black (RS, SC, PR, SP, RJ, ES, MG, GO, MS, MT, DF) from states in grey and light grey (AC, AM, RR, PA, AP, MA, PI, CE, RN, PB, PE, AL, SE, TO, BA).

²²Table A2 in the appendix provides estimates for each percentile of distance-bandwidths around state borders while restricting also to a small bandwidth of 30 days around the entry transition. Results are consistent and provide similar conclusions as above.

1.5.1.3 Alternative identification: Using DST extension

In this section we provide estimates of our main results using an alternative identification strategy. This strategy is somewhat related to the one adopted by Doleac and Sanders (2015) and Smith (2016) who used variation created by a 2007 legislated three-week period extension of DST in the US. In our case, we exploit a three-week variation in the start of DST between the DST terms of 2006-2007 and 2007-2008 (as previously shown in table 1) to identify the effect of interest. This change creates a range of dates that are standard time in 2006 and DST in 2007. We use this variation to estimate the following model

$$(4) \quad \begin{aligned} H_{ic} = & \beta_0 + \beta_1 DSTNew_{ic} \times Year_{2007} + \beta_2 Year_{2007} \\ & + \beta_3 DSTNew_{ic} + WeekDay_i + \lambda_c + \epsilon_{ic} \end{aligned}$$

where H_{ic} , $WeekDay_i$, λ_c are defined as above, $Year_{2007}$ is a dummy variable that equals 1 for the year of 2007 and 0 for the year of 2006 (year fixed effect), and $DSTNew_{ic}$ is an indicator variable that assumes value equal to one for the three-week period of DST extension, i.e., the period between October 14th (the new 2007 DST entry date) and November 5th. Note that in this specification we use only data for the years of 2006 and 2007 and consider only states that adopted DST. The parameter of interest is β_1 and measures the difference in the homicide rate between the three-week period following the 14th of October and the three-week period preceding the 14th of October for the years of 2007 and 2006. The identifying assumption here is that in absence of DST transition on October 14th of 2007, homicides in the three weeks following DST entry in this year would have had a similar trend as that observed in the same period for the year of 2006.

Results are presented in Figure 8. Again, we find the effect to be concentrated on the hours around sunset. All other estimates are statistically insignificant. The magnitude of the parameter estimated using sunset hours is the same as that obtained by our main empirical strategy, adding further credibility to our causal claim. Note that, since

we estimate this model using only the sample of states that adopt DST and consider a small window of three-weeks around the 2007 DST transition date, differences in the duration of daylight throughout the day caused by geographic features is likely to be of small importance, especially since differences are taken within municipalities for the same time (weeks) of the year. The estimates presented here imply a reduction of about 33.4% in homicides for the hours around sunset.

[Figure 8 about here.]

1.5.2 Fall back: transition to standard time

Thus far, we have provided compelling evidence that the transition from standard time to daylight saving time impacts significantly the homicide rate in Brazil. Now we turn to check if the transition from DST back to standard time causes homicides to increase. More importantly, we want to check if we observe significant changes in homicides around the hours that are mostly affected by the policy change. This would add strong evidence that indeed we can uncover the causal effect of the DST policy on homicides and that the main driving mechanism behind this relationship would be the variation in light induced by the shift in clock time.

Following the same methodological steps described above, in figure 9 we present difference-in-differences estimates of the effect of leaving DST by each group of hours of the day. Remarkably, results are the opposite of that obtained when considering the entrance transition. We find estimates that are either statistically insignificant or positive, when significant. The size of the coefficient estimated for the sunset hours is the same in magnitude, but of opposite sign, of that obtained when estimating the entry transition to DST. Taken as a whole, the transition from DST back to Standard Time increases the number of homicides in the same way as the one observed when entering DST and, more importantly, the shift in ambient light appears to be the driving mechanism through which homicides are impacted.

[Figure 9 about here.]

We note here that the transition back from DST to ST coincides with the period around the annual Brazilian Carnival. Carnival occurs seven Sundays before Easter, which, as a basic rule, usually falls on the first Sunday after the full moon following the spring equinox in the northern hemisphere. This introduces difficulties in isolating the effect of the carnival on homicides from that of DST on homicides. For instance, in some years the carnival happens right before the DST to ST transition, and in some, right after. This is further complicated by the fact that in some regions of Brazil Carnival festivities start a few weeks before the actual Carnival. This adds a lot of noise to weekly/daily measures of homicides. At the hourly level, however, we are still able to trace a significant increase in the homicide rate for the hours around sunset, those more likely to be affected, according to our theory and to the estimates presented above. In figure 10 we plot the estimated coefficients on event time for the sunset hours along with its 95 percent confidence intervals. We find a sharp increase in the homicide rate just after the transition from DST to standard time.

[Figure 10 about here.]

1.5.3 Dynamics dissection: RDD

Our strategies so far have relied on the common-trend assumption to infer the effects of the policy and its potential mechanisms. In this section we employ a regression discontinuity design to estimate short-term effects of the transition to DST on homicides under weaker assumptions.

In figure 11 we plot RD point estimates along with 95% confidence intervals for regressions restricted to two-hour periods around the standardized sunset hour. In panel A we plot estimates considering only states that adopted DST, while in panel B we plot estimates for those who did not adopt the policy. Accordingly, we attribute the exact DST entry dates of adopter states to non-adopter states in a falsification exercise as if they were experiencing a DST transition. If our story is right, we expect other factors affecting homicides (besides DST) to evolve smoothly around the transition date in states that did not adopt the policy.

We find estimates to be quantitatively close to zero for all hours, but those around sunset, for states that experienced a transition. The point estimate for the hours around sunset is $-.03$, suggesting a large short-term decrease in the homicide rate. For states that did not adopt the policy, estimates are not statistically different from zero, including those for the hours around sunset, for which we find a positive but very small coefficient. Note that here we compare the homicide rate for the same state just before the transition date with that just after to estimate the effect of the shift in clock time.

[Figure 11 about here.]

1.5.4 Robustness

1.5.4.1 Deaths before hospital admission

An important remark relates to whether increased mortality during the hours most affected by DST results from a lagged effect of higher crime levels in early hours that ended up being spuriously captured by our regressions. For instance, it would not be surprising if victims that got shot several hours before those most affected by the shift in lightness (sunset) ended up dying during sunset hours as a consequence of overcrowded hospitals, reduced hospital staff (caused by work shift friction due to DST), or a combination of both factors.²³

Unfortunately, we are not able to match the time of death of deceased patients to the time they were admitted to the hospital in order to check for this potential spurious effect. To tackle this issue, however, we estimate the effect around sunset as in the previous tables but restricting the sample to observations for which victims died before

²³Another possibility is that the transition to DST could affect the survival probability of victims, either by quicker reporting and/or response by rescue and ambulance services. This does not harm our interpretation that ambient light during evening hours reduce the number of homicides, but raises the possibility of not being directly the result of reduced criminal activity. Unfortunately, we do not dispose of a data set at the national level that contains criminal data (robbery, theft, etc) at the hourly/daily level. We show, however, using monthly data from the SINESPJC (*Sistema Nacional de Estatísticas de Segurança Pública e Justiça Criminal*), compiled from police records of criminal occurrences, that the number of car thefts decreased significantly during the three months of DST. Estimates for car robbery and for robbery followed by death are large and negative, but statistically insignificant. These estimates altogether suggest that criminal activity do respond to the shock.

an eventual hospital admission, thus eliminating the possibility of unrelated causes being driving our results. This is a conservative estimate, since we lose observations for which the victim got shot, was quickly admitted to a hospital and ended up dying briefly after admission.

Table 5 reports estimates of the effect of the policy on homicides for the whole day and for those hours around sunset. Overall, results are quite similar to those presented in table 4. We find that the number of victims that were declared dead before being admitted to hospital decrease by 11.82%, quantitatively similar to the reduction observed in homicides in general.

[Table 5 about here.]

1.5.4.2 Homicides by any mean or cause of death

To compare the consistency of our main outcome with a broader measure, in table 6 we test our main specification using deaths resulting from injuries inflicted by another person, by any means, with intent to injure or kill.²⁴ The distinction here is twofold: Differently from our main outcome, now we include homicides by all means - i.e. not only by firearm discharge but also by sharp object, blunt objects, bodily force and so on. Moreover, this new definition excludes cases of undetermined intent, although it is advisable not to do so (refer to Cerqueira, 2012, 2013, , as discussed in the introduction).

[Table 6 about here.]

In absolute terms, results are virtually equal to those presented in table 4, with negligible differences at the third decimal place. In relative terms, however, sunset results with our main outcomes are slightly greater than those using the alternative response variable. We suppose that not only most homicides in Brazil involve firearms but prohibition to carry makes homicides by this mean more deterrable by the supposed mechanisms.

²⁴Here we also exclude deaths from legal intervention.

1.5.4.3 Excluding day of transition

As a final check, we test whether a substantial part of the estimated effect is consequence of the day of transition being shorter - it has 23 hours instead of 24, due to the one-hour shift forward in clock-time. In this scenario, a share of the effect would be simply due to measurement error, as by construction a shorter day poses less time for homicides to take place.

As shown thus far, different hours of the day have different dynamics in homicides. Therefore, instead of performing adjustments such as imputing the average hourly homicide rate to that lost first hour, as has been previously done (see, for instance, Janszky and Ljung, 2008; Smith, 2016), we perform a “donut” regression, which excludes observations in the exact day of the transition, following a rationale discussed in Barreca et al. (2011). If the underlying hypotheses are valid, although we may incur a loss of precision due to less information being available in the sample, we expect to find point estimates similar to those presented for our main specification (table 4), which is exactly what we see in table 7. Results are quite stable and show that homicides decrease by 6.18-9.31% after transition.

[Table 7 about here.]

1.5.5 Heterogeneity

1.5.5.1 Decomposition urban vs rural areas

In this section we explore if urban and rural municipalities respond differently to the transition to DST. On one hand, municipalities that have a larger share of the population living in urban settings may benefit less from the additional ambient light if artificial light is widespread throughout the more densely populated area. On the other hand, if in urban areas the perception of law enforcement is felt more strongly, as potential criminals believe they have a higher arrest probability, then we might observe a larger effect of the policy on homicides as the shift in ambient light will deter criminals from taking action.

In table 8 we present estimates of the effect of the policy throughout the whole DST period for daily homicides (Panel A) and for homicides that occur on the sunset hours (Panel B). From columns 1 through 9 we estimate the model restricting the sample for percentiles of the share of urban population calculated for each municipality. For instance, the 10th percentile corresponds to a 33.9% share of urban population in the municipality, while the 90th percentile corresponds to having 93.1% of the population living in urban areas.

We find that the effect of the policy is felt on rural areas as well as on more urbanized regions. The magnitude of the coefficient is slightly larger when considering municipalities that have a smaller share of the population living in urban areas. Since more urbanized areas usually have a level of criminal activity that is significantly larger than that observed in rural environments, the percentage drop in homicides during DST period is much smaller when looking at daily homicides. For instance, we observe a 20% drop in homicides for the lowest percentile of share of urban population and a 6.8% decline for the 90th percentile. For sunset hours, we find similar effects for both urban and rural areas.

[Table 8 about here.]

1.6 Concluding remarks and implications

Isolating the effect of policies on crime-deterrence is often a challenging task both due to non-random measurement error in crime outcomes and to compound effects that may come hand in hand with the policy of interest. In this paper we exploit an abrupt change in light hours, caused by the transition from Standard to Daylight Saving Time, to measure the overall effect of this policy and its main mechanism - the effect of light - on homicide occurrence. We take advantage of the institutional framework of this policy in Brazil which, in contrast to the U.S., contemporaneously divides the country into a treatment and a control regions based on technical reports from the National Electric System Operator, and enforced by Federal enactments.

We use hourly data on homicides by firearm discharge provided by the Brazilian Ministry of Health and exploit the discontinuity in treatment status for days around the transition date to assess the extent to which lightness during otherwise dark hours can deter homicidal interactions. Our estimates indicate that the shift in light during early evening hours reduced the incidence of homicides by roughly 9.83% when comparing treated to untreated states. In line with the crime deterrence hypothesis, we show that this effect is concentrated exactly within the postulated hours. We provide an extensive set of robustness checks. Based on the estimated overall effect of the policy, back-of-the-envelope calculations suggest the shift in ambient light was responsible for saving about 5,035 potential victims from 2006-2015.

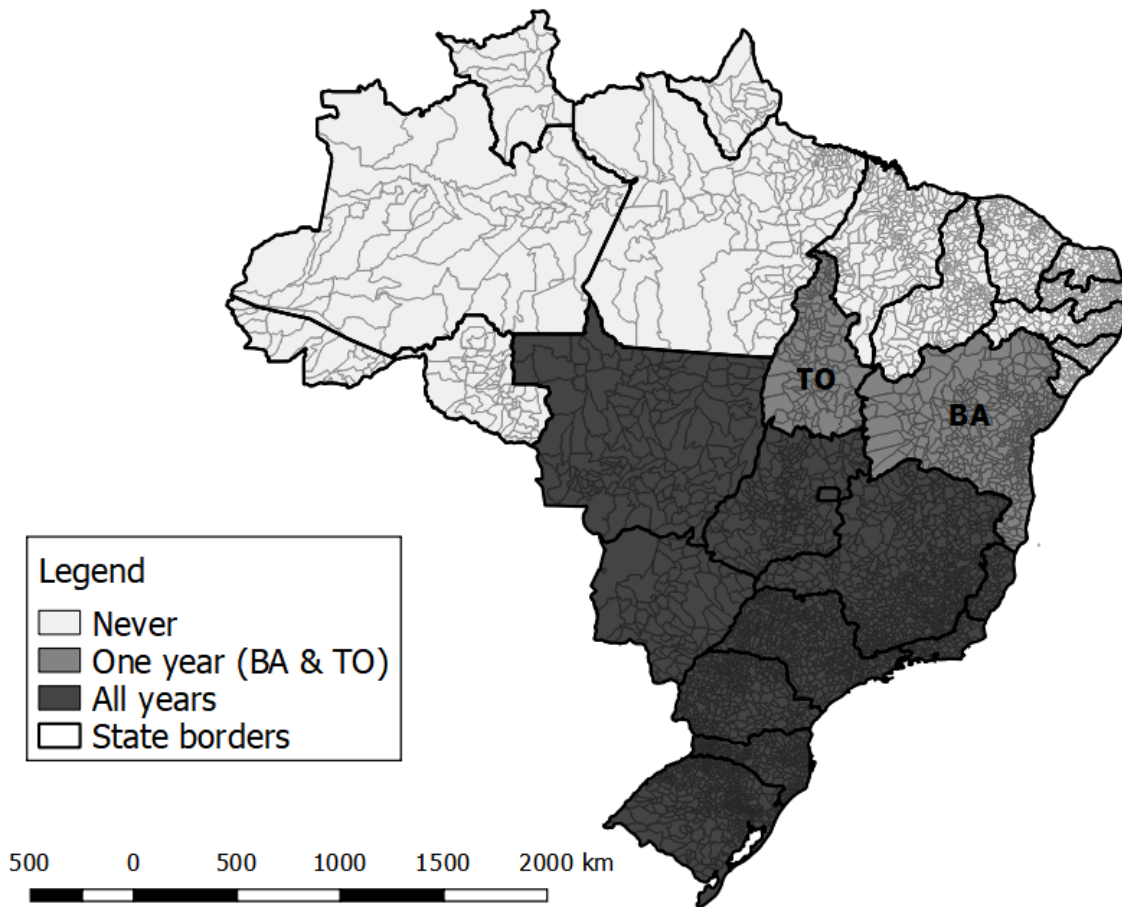
These results however should be taken with some reservations regarding other unanticipated consequences of the policy in question, although those are not related to the channels analyzed in this paper. While on one hand monetary and social costs of violence are mitigated, on the other recent literature suggests that metabolic disturbances caused by this shift in clock-hours with respect to solar time, jointly with one hour of sleep lost in the transition day, are responsible for a set of undesirable outcomes ranging from reduced individual well-being (Kountouris and Remoundou, 2014) to increased incidence of heart attack (Janszky and Ljung, 2008; Toro, Tigre and Sampaio, 2015), and increased risk of traffic accidents (Smith, 2016).

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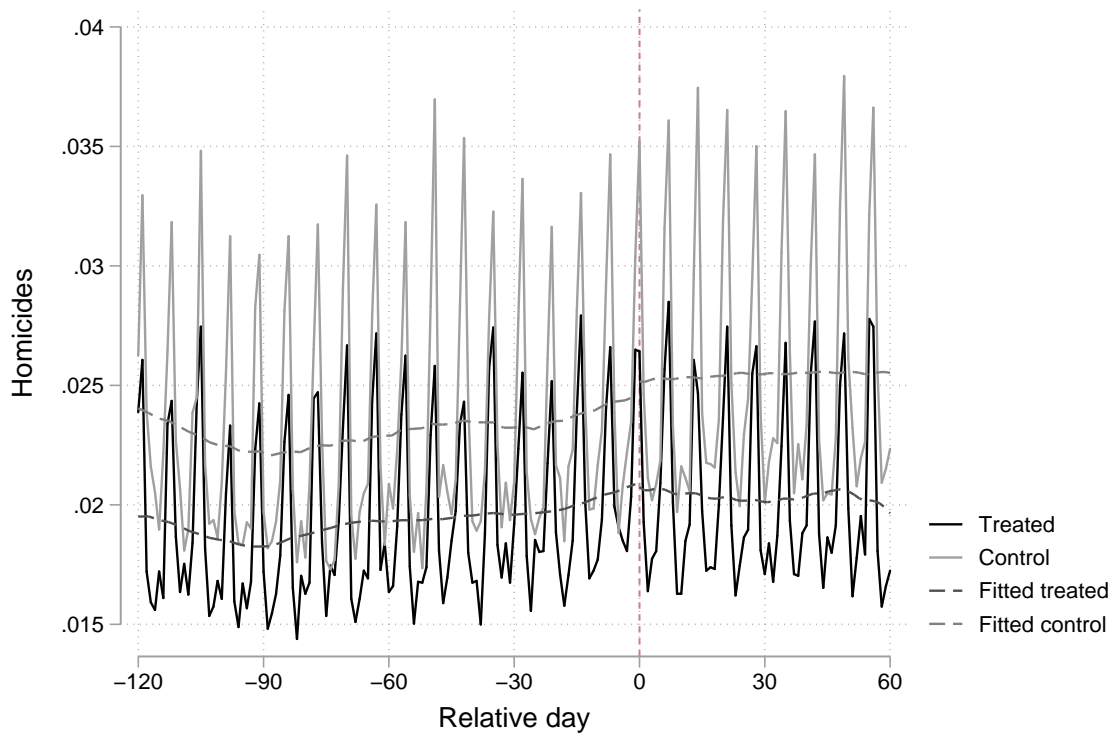
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Figure 1: DST policy in Brazil: 2006-2015



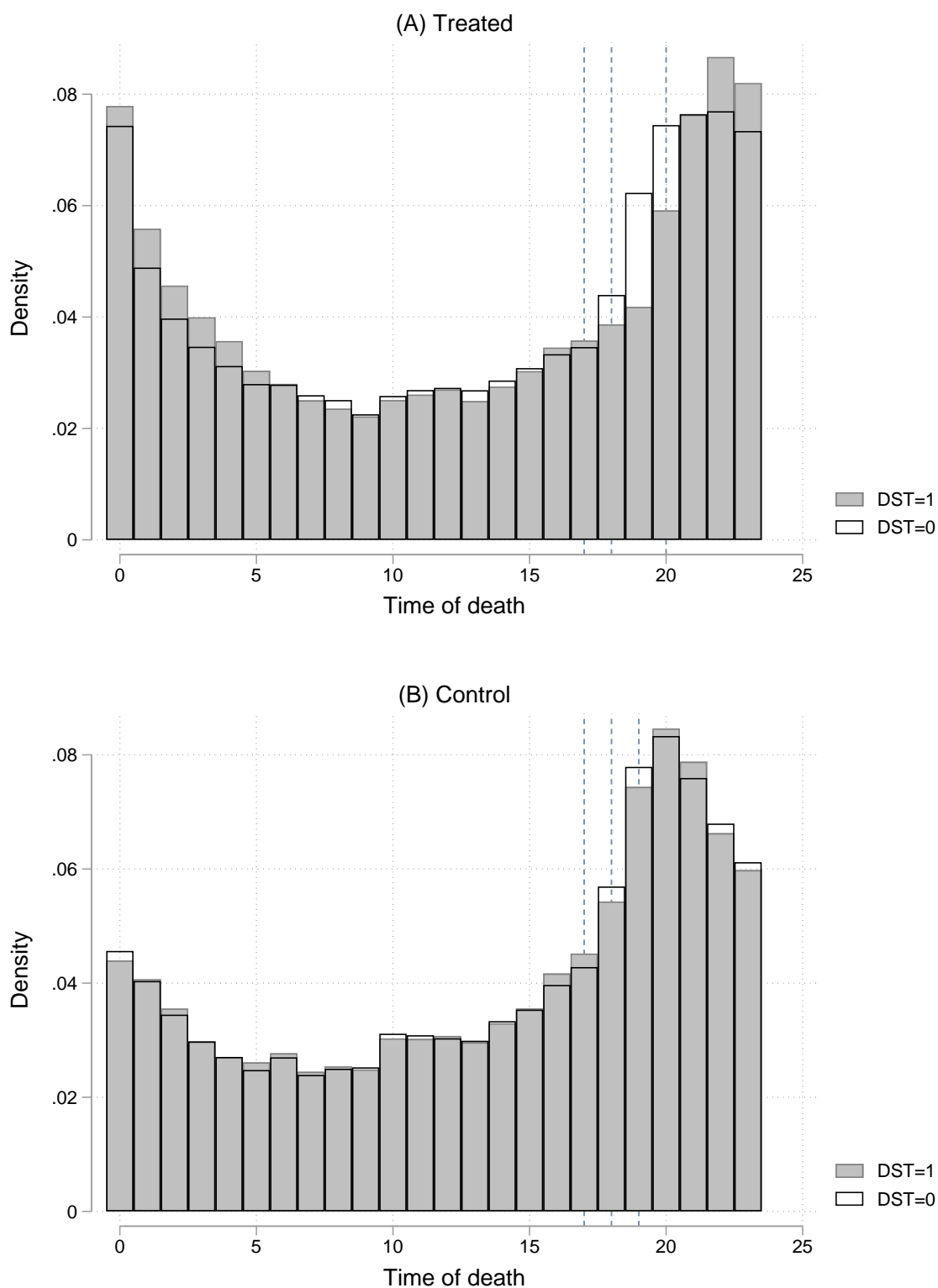
Note: This figure presents the geographic distribution of DST states in Brazil. States dark gray adopted DST in all years of the sample. States in gray adopted DST once (Bahia in the DST 2011-2012 and Tocantins in the DST 2012-2013). States in light gray never adopted DST.

Figure 2: Raw data: Dynamics of daily homicides for treated and control regions



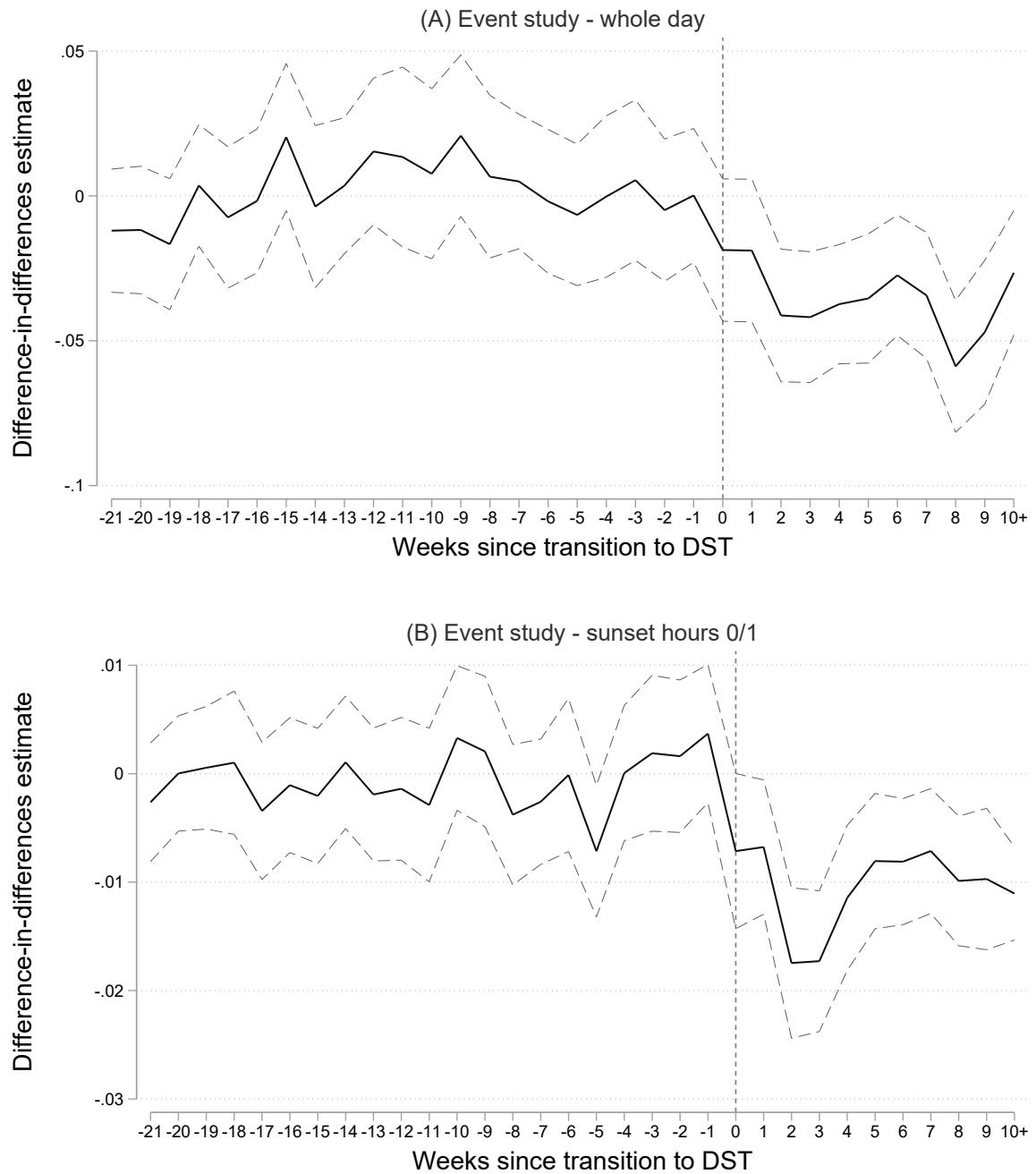
Note: This figure plots homicides around the transition to DST. Each bin represents average of municipal-level homicides count aggregated by treatment group and day relative to the transition dates, 0 being the day of transition. Dashed lines represent fitted polynomial splines with no controls for each side of the transition date.

Figure 3: Raw data: Distribution of homicides by hour of the day during DST and standard time



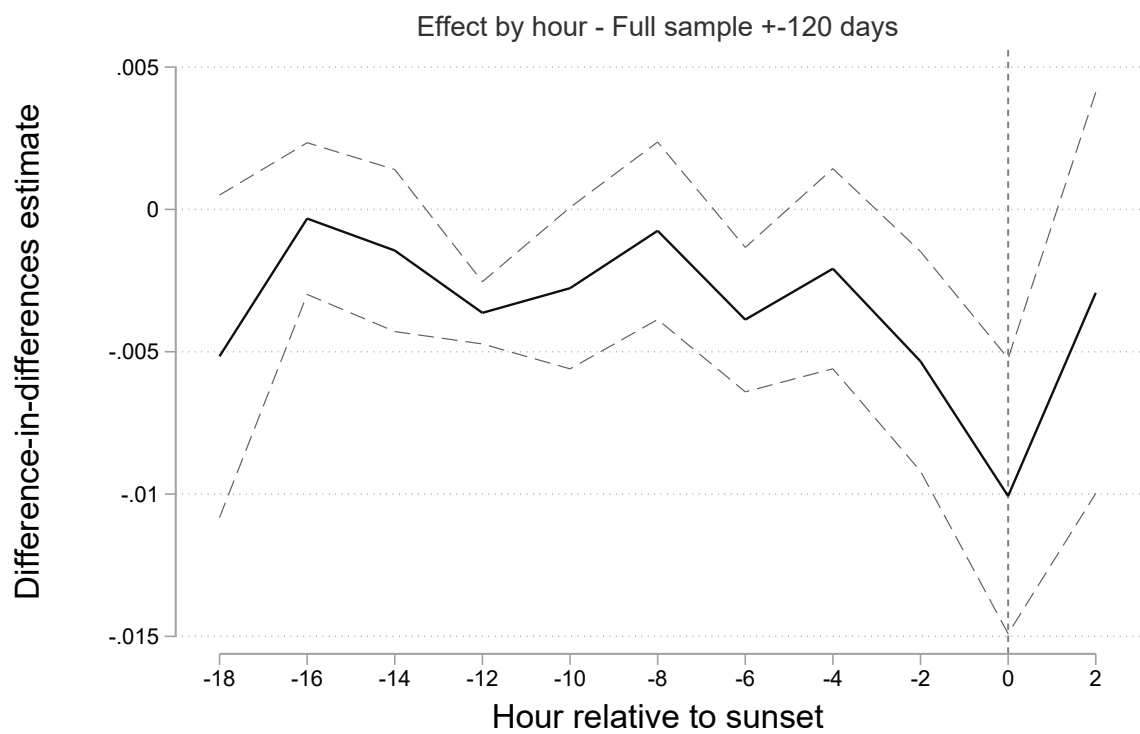
Note: This figure presents the distribution of homicides by hour of the day. Gray bars represent the density of homicides in a given hour during the DST period whereas white bars refer to the standard time period. For each treatment group, vertical dashed lines represent the minimum, mean, and maximum hours of standard-time sunset for municipalities.

Figure 4: Full sample: Difference-in-differences estimates by week since transition to DST



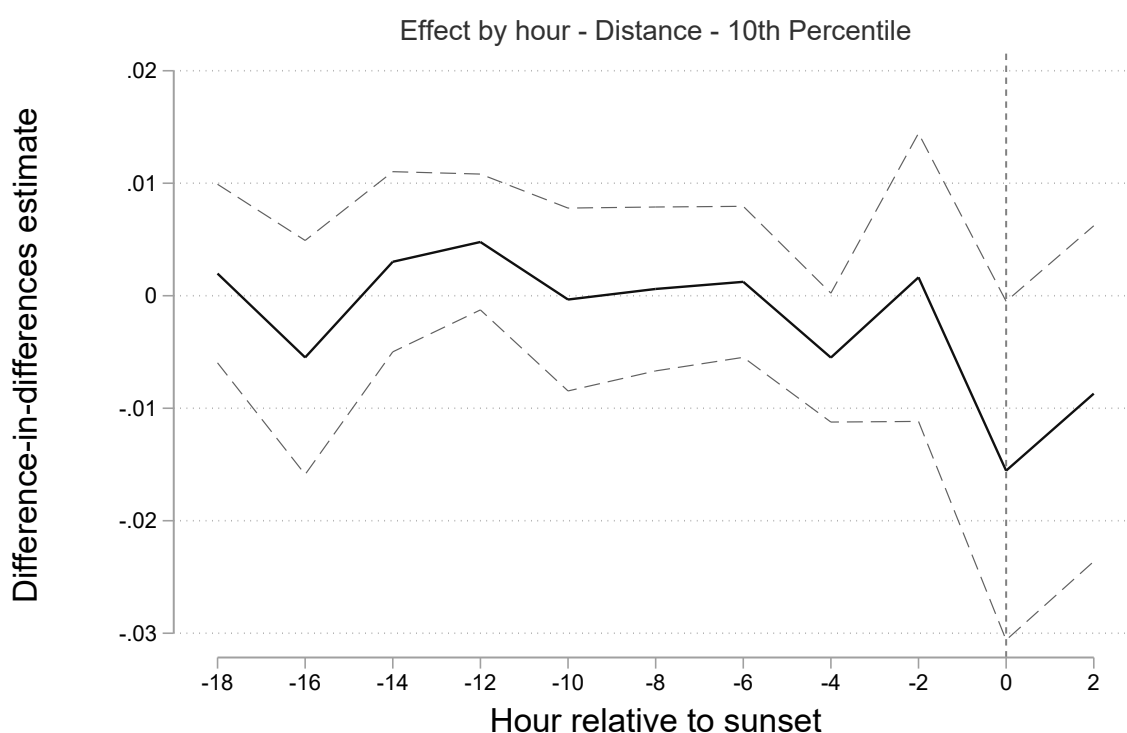
Note: This figure presents difference-in-differences estimates in the fashion of leads and lags for weeks since transition to DST. The specifications contain municipality fixed effects, day-of-week fixed effects, day-of-month fixed effects, and year fixed effects. Standard errors are clustered at the municipal level.

Figure 5: Full sample: Difference-in-differences estimates for each hour-group



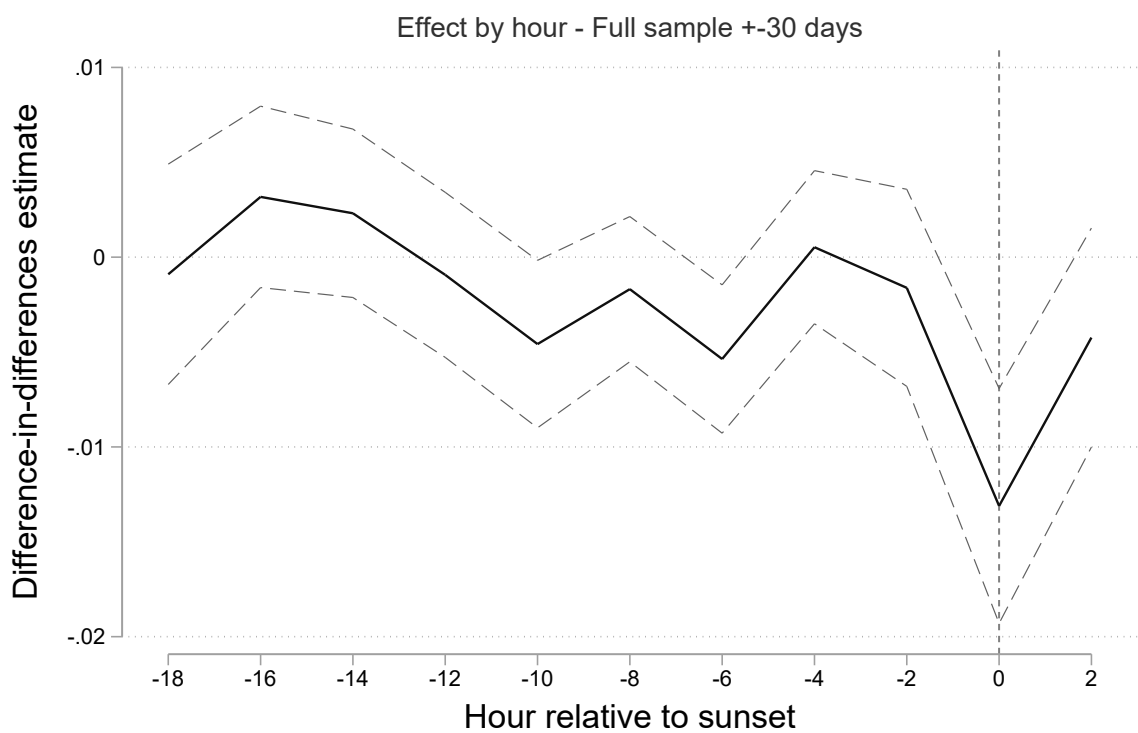
Note: This figure presents difference-in-differences estimates by each group of hours of the day. The specifications contain municipality fixed effects, day-of-week fixed effects, day-of-month fixed effects, and year fixed effects. Standard errors are clustered at the state level.

Figure 6: Difference-in-differences estimates for each hour-group for municipalities within a band of 190km from each side of the border that divides treatment and control groups



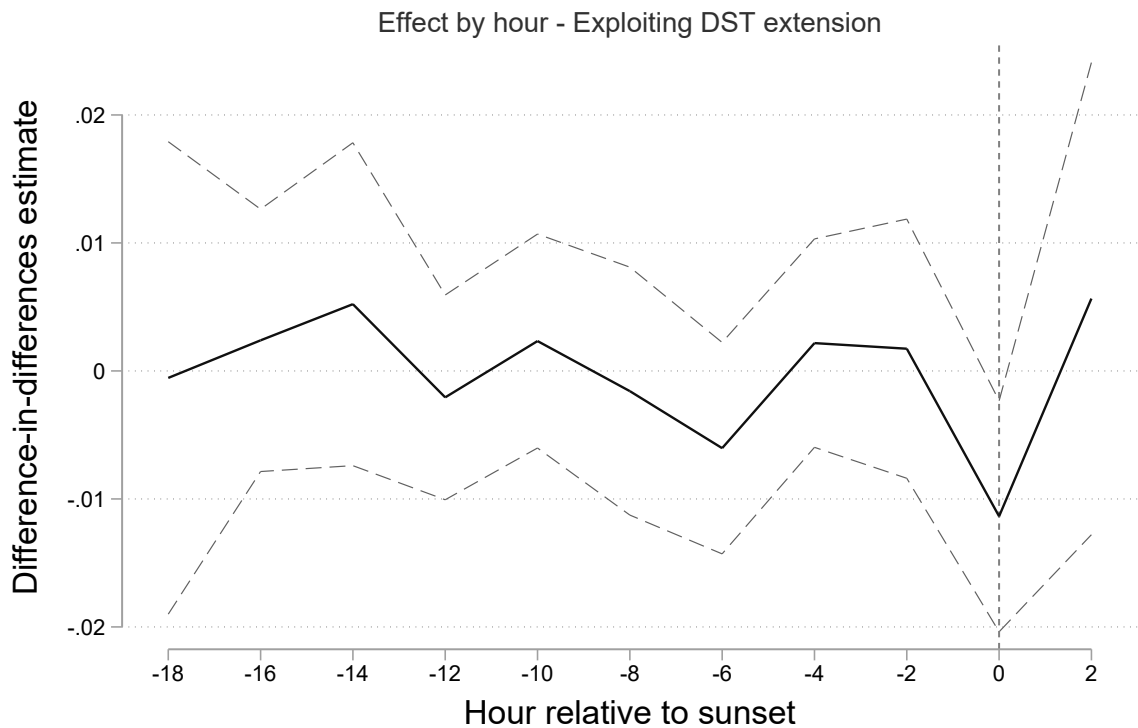
Note: This figure presents difference-in-differences estimates by each group of hours of the day while restricting the sample used in figure 5 to municipalities within a band of 190km (10th percentile of the distance-to-border distribution) from each side of the border that divides treatment and control groups. The specifications contain municipality fixed effects, day-of-week fixed effects, day-of-month fixed effects, and year fixed effects. Standard errors are clustered at the state level.

Figure 7: Difference-in-differences estimates for each hour-group for a window of ± 30 days



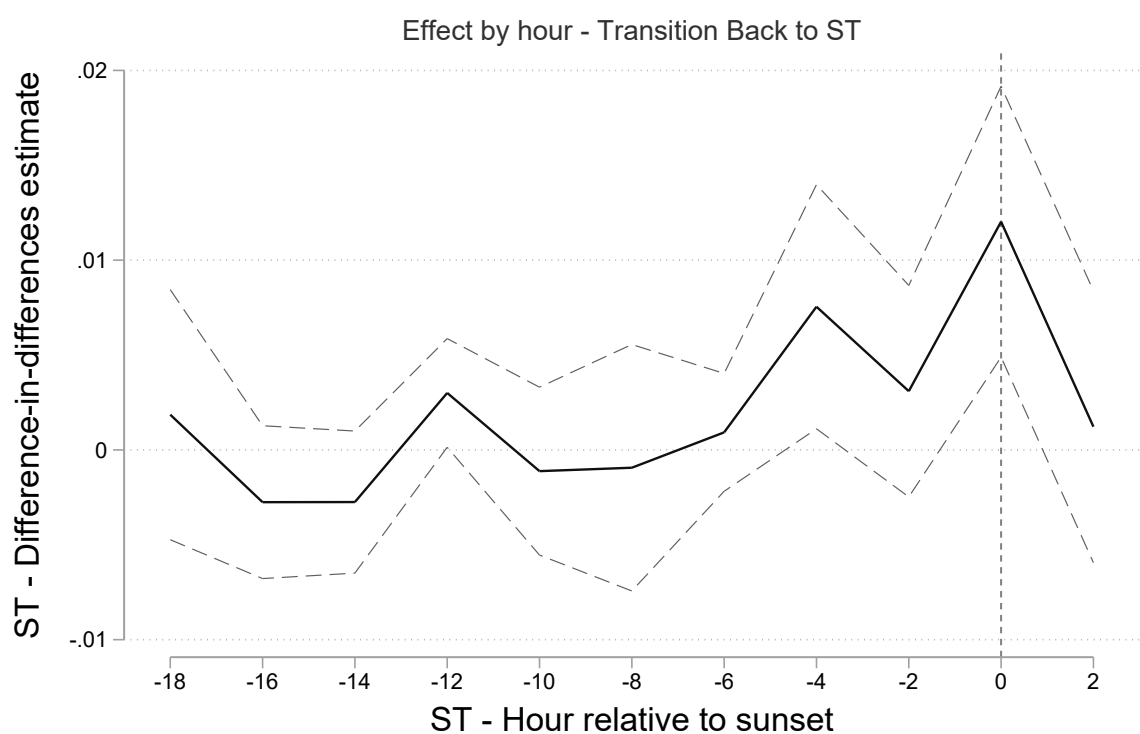
Note: This figure presents difference-in-differences estimates by each group of hours of the day in a window of ± 30 days around DST transition. The specifications contain municipality fixed effects, day-of-week fixed effects, day-of-month fixed effects, and year fixed effects. Standard errors are clustered at the state level.

Figure 8: Alternative identification - Difference-in-differences estimates for each hour-group exploiting DST extension



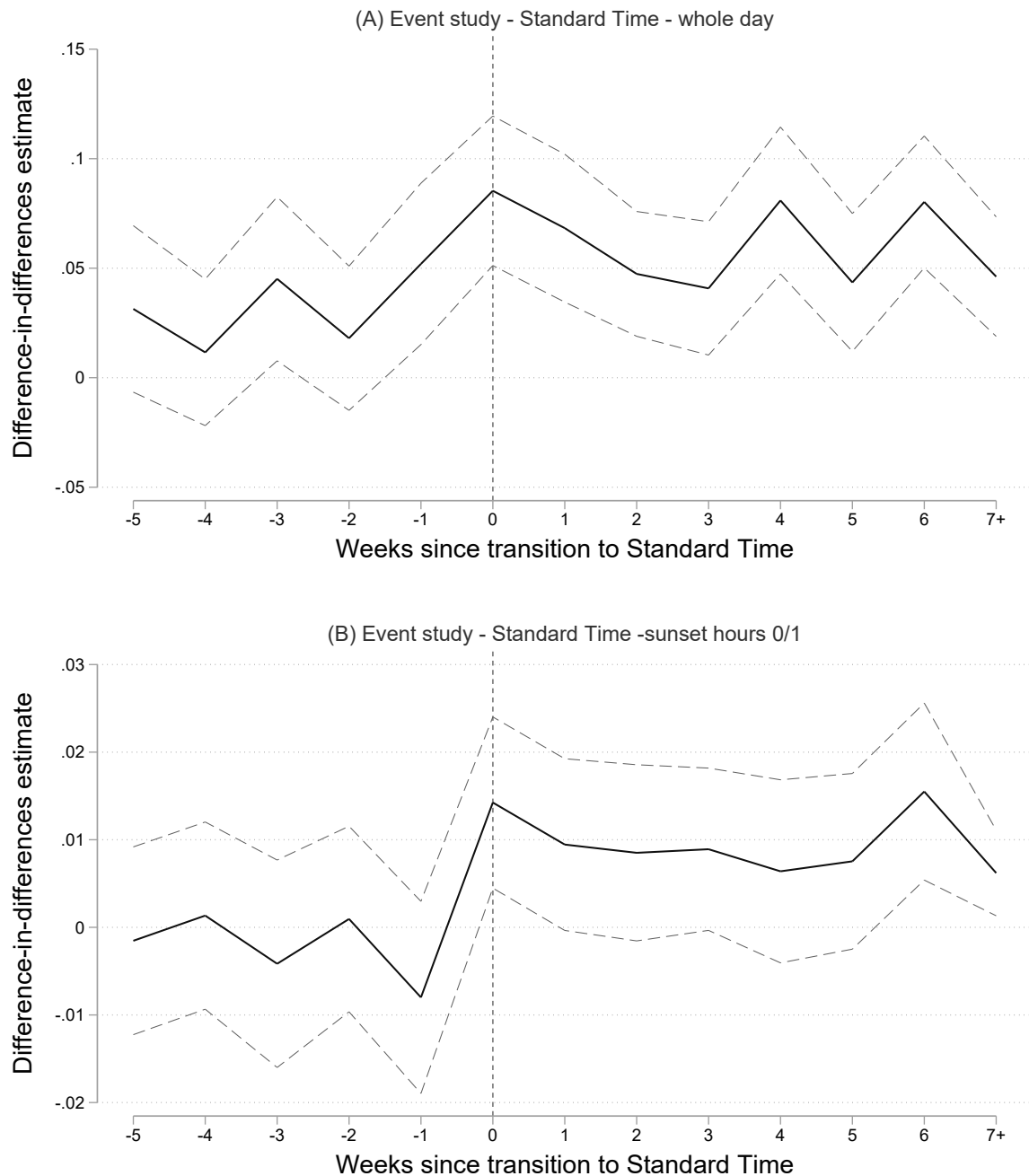
Note: This figure presents difference-in-differences estimates by each group of hours of the day while restricting the sample used in figure 5 only to treated municipalities and exploiting variation in the duration of DST between the DST terms 2006-2007 and 2007-2008, in the same fashion of Doleac and Sanders (2015) and Smith (2016). The specifications contain municipality fixed effects, day-of-week fixed effects, day-of-month fixed effects, and year fixed effects. Standard errors are clustered at the municipality level.

Figure 9: Transition from DST to ST - Difference-in-differences estimates for each hour-group



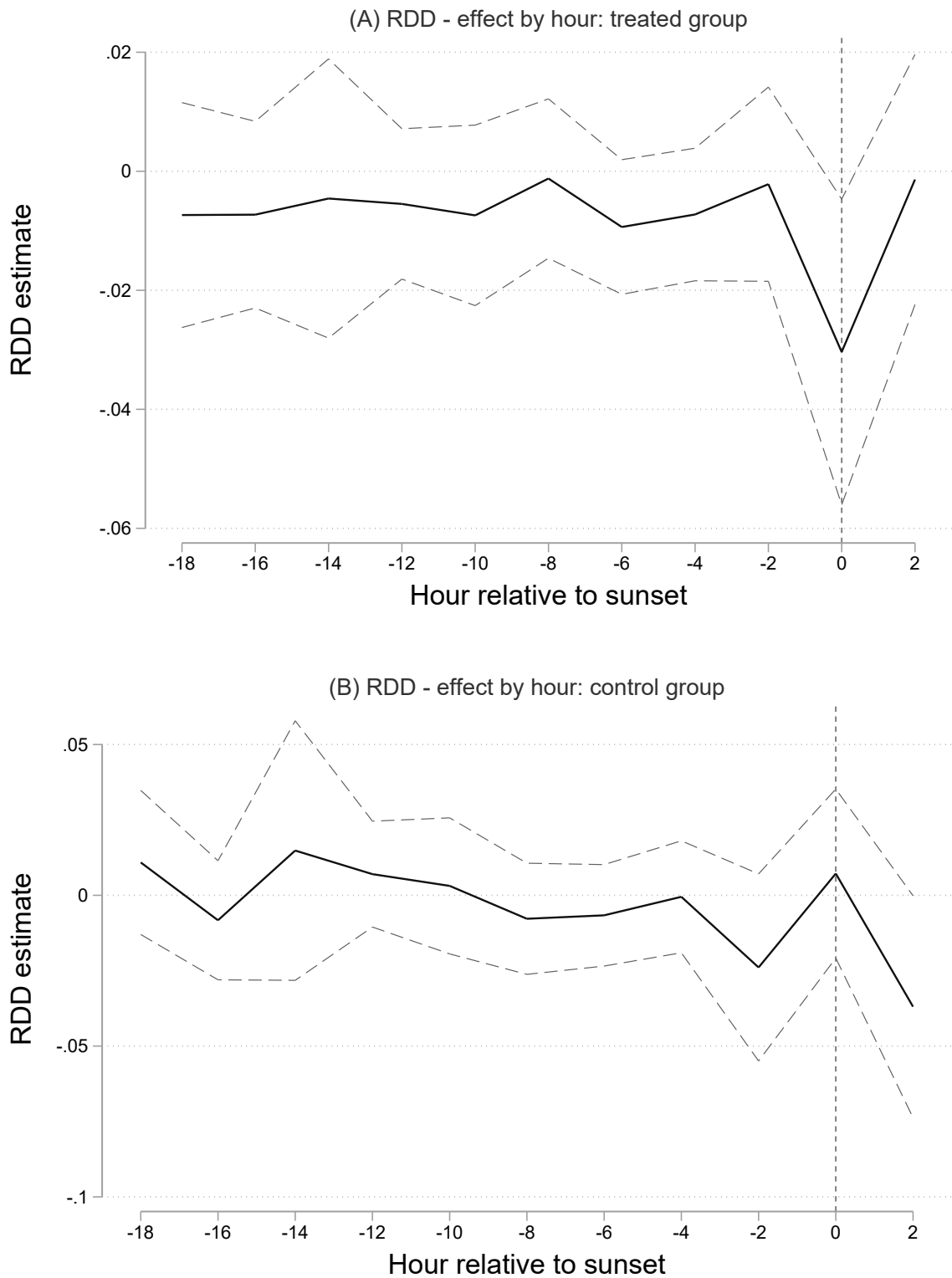
Note: This figure presents difference-in-differences estimates of the effect of *leaving* DST by each group of hours of the day. The specifications contain municipality fixed effects, day-of-week fixed effects, day-of-month fixed effects, carnival days fixed effects, and year fixed effects. Standard errors are clustered at the state level.

Figure 10: Transition from DST to ST: Difference-in-differences estimates by week since transition back to ST



Note: This figure presents difference-in-differences estimates in the fashion of leads and lags for weeks since transition to ST (i.e. leaving DST). The specifications contain municipality fixed effects, day-of-week fixed effects, day-of-month fixed effects, and year fixed effects. Standard errors are clustered at the municipal level.

Figure 11: RDD - Within-differences estimates for each hour-group



Note: This figure presents RDD estimates disaggregated by treatment and control groups by each group of hours of the day. The specifications contain municipality fixed effects, day-of-week fixed effects, day-of-month fixed effects, and year fixed effects. Standard errors are clustered at the state level

Table 1: Treated states

Year	Begin	End	Length (days)	States
2006-2007	Nov 5 2006	Feb 25 2007	112	DF, ES, GO, MG, MS, MT, PR, RJ, RS, SC, SP.
2007-2008	Oct 14 2007	Feb 17 2008	126	DF, ES, GO, MG, MS, MT, PR, RJ, RS, SC, SP.
2008-2009	Oct 19 2008	Feb 15 2009	119	DF, ES, GO, MG, MS, MT, PR, RJ, RS, SC, SP.
2009-2010	Oct 18 2009	Feb 21 2010	126	DF, ES, GO, MG, MS, MT, PR, RJ, RS, SC, SP.
2010-2011	Oct 17 2010	Feb 20 2011	126	DF, ES, GO, MG, MS, MT, PR, RJ, RS, SC, SP.
2011-2012	Oct 16 2011	Feb 26 2012	133	BA, DF, ES, GO, MG, MS, MT, PR, RJ, RS, SC, SP.
2012-2013	Oct 21 2012	Feb 17 2013	119	DF, ES, GO, MG, MS, MT, PR, RJ, RS, SC, SP, TO.
2013-2014	Oct 20 2013	Feb 16 2014	119	DF, ES, GO, MG, MS, MT, PR, RJ, RS, SC, SP.
2014-2015	Oct 19 2014	Feb 22 2015	126	DF, ES, GO, MG, MS, MT, PR, RJ, RS, SC, SP.
2015-2016	Oct 18 2015	Feb 21 2016	126	DF, ES, GO, MG, MS, MT, PR, RJ, RS, SC, SP.

Note: This table presents the list of the Brazilian states that adopted DST from 2006-2015. It also reports the date of transition from standard time (ST) to DST and from DST to ST. Source: <http://www.mme.gov.br/>. State codes: BA - Bahia; DF - Distrito Federal; ES - Espírito Santo; GO - Goiás; MG - Minas Gerais; MS - Mato Grosso do Sul; MT - Mato Grosso; PR - Paraná; RJ - Rio de Janeiro; RS - Rio Grande do Sul; SC - Santa Catarina; SP - São Paulo; TO - Tocantins.

Table 2: ICD-10 codes–deaths from firearm discharge

Code	Description	Frequency	%
X93	Assault by handgun discharge	25,300	9.26
X94	Assault by rifle, shotgun and larger firearm discharge	2,009	0.74
X95	Assault by other and unspecified firearm discharge	238,192	87.19
Y22	Handgun discharge, undetermined intent	73	0.03
Y23	Rifle, shotgun and larger firearm discharge, undetermined intent	68	0.02
Y24	Other and unspecified firearm discharge, undetermined intent	7,539	2.76

Note: This table presents the list of ICD-10 codes considered in the construction of the dependent variable used for the main results.

Table 3: Average death-rate by treatment group - weeks around transition to DST

Group	Whole day			Sunset		
	Week Pre-DST (1)	Week Post-DST (2)	Difference (3)	Week Pre-DST (4)	Week Post-DST (5)	Difference (6)
Treated	0.467 (2.991)	0.437 (3.014)	-0.030*** (0.009)	0.053 (1.070)	0.042 (1.083)	-0.010*** (0.003)
Control	0.791 (4.152)	0.778 (3.910)	-0.012 (0.015)	0.101 (1.498)	0.099 (1.398)	-0.002 (0.005)

Note: This table presents the average homicide rate, decomposed by treatment status, for a week before the transition date and a week after. The sample size for Treated and Control are, respectively, 3492 and 2169 municipalities - summing up to 5661, since some enter as Control in most years and treated in a single year (BA and TO). Averages are weighted by municipal population from the 2010 census. ***, ** and * represent $p < 1\%$, $p < 5\%$ and $p < 10\%$ respectively.

Table 4: Difference-in-differences estimates: different temporal bandwidths around the transition date

(A) <i>Whole day</i>	20 days (1)	30 days (2)	60 days (3)	120 days (4)	120 days (5)
DST	-0.0251* (0.012)	-0.0288** (0.013)	-0.0289** (0.012)	-0.0288** (0.014)	-0.0345** (0.015)
Municipality F.E	YES	YES	YES	YES	YES
Day-of-week F.E	YES	YES	YES	YES	YES
Day-of-month F.E	YES	YES	YES	YES	YES
Election	YES	YES	YES	YES	YES
Carnival	NO	NO	NO	YES	YES
Year	YES	YES	YES	YES	YES
Municipal trend	YES	YES	YES	YES	NO
Baseline mean	.3067	.3061	.3003	.2933	.2933
% Δ from baseline	-8.183	-9.413	-9.618	-9.83	-11.767
Municipalities	5112	5112	5112	5112	5112
Observations	2,044,800	3,067,200	6,134,400	11,982,528	11,982,528
(B) <i>Hours 0/1</i>	20 days (1)	30 days (2)	60 days (3)	120 days (4)	120 days (5)
DST	-0.0113*** (0.003)	-0.0129*** (0.003)	-0.0093*** (0.002)	-0.0093*** (0.002)	-0.0101*** (0.002)
Municipality F.E	YES	YES	YES	YES	YES
Day-of-week F.E	YES	YES	YES	YES	YES
Day-of-month F.E	YES	YES	YES	YES	YES
Election	YES	YES	YES	YES	YES
Carnival	NO	NO	NO	YES	YES
Year	YES	YES	YES	YES	YES
Municipal trend	YES	YES	YES	YES	NO
Baseline mean	.0365	.0355	.0338	.0322	.0322
% Δ from baseline	-31.019	-36.223	-27.601	-28.817	-31.274
Municipalities	5112	5112	5112	5112	5112
Observations	2,044,800	3,067,200	6,134,400	11,982,528	11,982,528

Note: This table presents difference-in-differences estimates of the effect of DST on homicide rate per million inhabitants for different time-spans around the transition to DST. Panel (A) *Whole day* presents estimates considering all hours of the day, whereas panel (B) *Hours 0/1* restrict the sample to sunset hours. Row *Baseline mean* displays the mean of the outcome for treated units during the pre-treatment period in question. Row % Δ from baseline displays the percent variation from $\hat{\beta}_1$ with respect to the baseline mean. Specifications include municipality fixed effects, day-of-week fixed effects, day-of-month fixed effects, year fixed effects and, when applicable, municipal-specific linear trends. Standard errors clustered at the state level are presented in parentheses. ***, ** and * represent $p < 1\%$, $p < 5\%$ and $p < 10\%$ respectively.

Table 5: Deaths before hospital admission

(A) <i>Whole day</i>	20 days (1)	30 days (2)	60 days (3)	120 days (4)
DST	-0.0192* (0.011)	-0.0188 (0.011)	-0.0212** (0.010)	-0.0227* (0.012)
Municipality F.E	YES	YES	YES	YES
Day-of-week F.E	YES	YES	YES	YES
Day-of-month F.E	YES	YES	YES	YES
Election	YES	YES	YES	YES
Carnival	NO	NO	NO	YES
Year	YES	YES	YES	YES
Municipal trend	YES	YES	YES	YES
Baseline mean	.2012	.2014	.197	.1921
% Δ from baseline	-9.557	-9.357	-10.784	-11.828
Municipalities	5112	5112	5112	5112
Observations	2,044,800	3,067,200	6,134,400	11,982,528
(B) <i>Hours 0/1</i>	20 days (1)	30 days (2)	60 days (3)	120 days (4)
DST	-0.0091*** (0.003)	-0.0100*** (0.003)	-0.0062*** (0.002)	-0.0077*** (0.002)
Municipality F.E	YES	YES	YES	YES
Day-of-week F.E	YES	YES	YES	YES
Day-of-month F.E	YES	YES	YES	YES
Election	YES	YES	YES	YES
Carnival	NO	NO	NO	YES
Year	YES	YES	YES	YES
Municipal trend	YES	YES	YES	YES
Baseline mean	.025	.0244	.0232	.022
% Δ from baseline	-36.401	-40.911	-26.687	-34.849
Municipalities	5112	5112	5112	5112
Observations	2,044,800	3,067,200	6,134,400	11,982,528

Note: This table presents difference-in-differences estimates of the effect of DST on homicide rate per million inhabitants while restricting the sample of Table 4 to deaths that occurred before admission to a health facility. The panel (A) *Whole day* presents estimates considering all hours of the day, whereas panel (B) *Hours 0/1* restricts the sample to sunset hours. Row *Baseline mean* displays the mean of the outcome for treated units during the pre-treatment period in question. Row % Δ from baseline displays the percent variation from $\hat{\beta}_1$ with respect to the baseline mean. Specifications include municipality fixed effects, day-of-week fixed effects, day-of-month fixed effects, year fixed effects and, when applicable, municipal-specific linear trends. Standard errors clustered at the state level are presented in parentheses. ***, ** and * represent $p < 1\%$, $p < 5\%$ and $p < 10\%$ respectively.

Table 6: Alternative outcome: death rate as consequence of injuries inflicted by another person, by any means, with intent to injure or kill

(A) <i>Whole day</i>	20 days (1)	30 days (2)	60 days (3)	120 days (4)
DST	-0.0349** (0.013)	-0.0292** (0.013)	-0.0287** (0.013)	-0.0280* (0.016)
Municipality F.E	YES	YES	YES	YES
Day-of-week F.E	YES	YES	YES	YES
Day-of-month F.E	YES	YES	YES	YES
Election	YES	YES	YES	YES
Carnival	NO	NO	NO	YES
Year	YES	YES	YES	YES
Municipal trend	YES	YES	YES	YES
Baseline mean	.4327	.436	.4269	.4185
% Δ from baseline	-8.058	-6.698	-6.729	-6.684
Municipalities	5112	5112	5112	5112
Observations	2,044,800	3,067,200	6,134,400	11,982,528
(B) <i>Hours 0/1</i>	20 days (1)	30 days (2)	60 days (3)	120 days (4)
DST	-0.0111*** (0.003)	-0.0120*** (0.004)	-0.0095*** (0.003)	-0.0106*** (0.003)
Municipality F.E	YES	YES	YES	YES
Day-of-week F.E	YES	YES	YES	YES
Day-of-month F.E	YES	YES	YES	YES
Election	YES	YES	YES	YES
Carnival	NO	NO	NO	YES
Year	YES	YES	YES	YES
Municipal trend	YES	YES	YES	YES
Baseline mean	.0478	.0474	.0461	.0443
% Δ from baseline	-23.123	-25.328	-20.646	-24.008
Municipalities	5112	5112	5112	5112
Observations	2,044,800	3,067,200	6,134,400	11,982,528

Note: This table presents difference-in-differences estimates of the effect of DST on homicide rate per million inhabitants as defined by death rate as consequence of injuries inflicted by another person, by any means, with intent to injure or kill. The panel (A) *Whole day* presents estimates considering all hours of the day, whereas panel (B) *Hours 0/1* restricts the sample to sunset hours. Row *Baseline mean* displays the mean of the outcome for treated units during the pre-treatment period in question. Row % Δ from baseline displays the percent variation from β_1 with respect to the baseline mean. Specifications include municipality fixed effects, day-of-week fixed effects, day-of-month fixed effects, year fixed effects and, when applicable, municipal-specific linear trends. Standard errors clustered at the state level are presented in parentheses. ***, ** and * represent $p < 1\%$, $p < 5\%$ and $p < 10\%$ respectively.

Table 7: Difference-in-differences estimates: excluding day of transition - different temporal-bandwidths around the transition date

(A) <i>Whole day</i>	20 days (1)	30 days (2)	60 days (3)	120 days (4)
DST	-0.0261* (0.014)	-0.0189 (0.013)	-0.0244** (0.012)	-0.0273* (0.014)
Municipality F.E	YES	YES	YES	YES
Day-of-week F.E	YES	YES	YES	YES
Day-of-month F.E	YES	YES	YES	YES
Election	YES	YES	YES	YES
Carnival	NO	NO	NO	YES
Year	YES	YES	YES	YES
Municipal trend	YES	YES	YES	YES
Baseline mean	.3067	.3061	.3003	.2933
% Δ from baseline	-8.521	-6.183	-8.119	-9.311
Municipalities	5112	5112	5112	5112
Observations	2,044,800	3,067,200	6,134,400	11,880,288
(B) <i>Hours 0/1</i>	20 days (1)	30 days (2)	60 days (3)	120 days (4)
DST	-0.0136*** (0.003)	-0.0121*** (0.003)	-0.0089*** (0.002)	-0.0091*** (0.002)
Municipality F.E	YES	YES	YES	YES
Day-of-week F.E	YES	YES	YES	YES
Day-of-month F.E	YES	YES	YES	YES
Election	YES	YES	YES	YES
Carnival	NO	NO	NO	YES
Year	YES	YES	YES	YES
Municipal trend	YES	YES	YES	YES
Baseline mean	.0365	.0355	.0338	.0322
% Δ from baseline	-37.375	-34.207	-26.42	-28.401
Municipalities	5112	5112	5112	5112
Observations	2,044,800	3,067,200	6,134,400	11,880,288

Note: This table presents difference-in-differences estimates of the effect of DST on homicide rate per million inhabitants for different time-spans around the transition to DST while excluding the day of transition. Panel (A) *Whole day* presents estimates considering all hours of the day, whereas panel (B) *Hours 0/1* restrict the sample to sunset hours. Row *Baseline mean* displays the mean of the outcome for treated units during the pre-treatment period in question. Row % Δ from baseline displays the percent variation from $\hat{\beta}_1$ with respect to the baseline mean. Specifications include municipality fixed effects, day-of-week fixed effects, day-of-month fixed effects, year fixed effects and, when applicable, municipal-specific linear trends. Standard errors clustered at the state level are presented in parentheses. ***, ** and * represent $p < 1\%$, $p < 5\%$ and $p < 10\%$ respectively.

Table 8: Difference-in-differences estimates: heterogeneity by percentile of urban population

(A) <i>Whole day</i>	10th pct (1)	20th pct (2)	30th pct (3)	40th pct (4)	50th pct (5)	60th pct (6)	70th pct (7)	80th pct (8)	90th pct (9)
DST	-0.0338* (0.019)	-0.0399*** (0.013)	-0.0298*** (0.010)	-0.0326*** (0.009)	-0.0323*** (0.008)	-0.0291*** (0.007)	-0.0208*** (0.007)	-0.0250*** (0.006)	-0.0159*** (0.007)
Municipality F.E	YES	YES	YES	YES	YES	YES	YES	YES	YES
Day-of-week F.E	YES	YES	YES	YES	YES	YES	YES	YES	YES
Day-of-month F.E	YES	YES	YES	YES	YES	YES	YES	YES	YES
Election	YES	YES	YES	YES	YES	YES	YES	YES	YES
Carnival	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year	YES	YES	YES	YES	YES	YES	YES	YES	YES
Municipal trend	YES	YES	YES	YES	YES	YES	YES	YES	YES
Baseline mean	.1689	.1664	.1867	.1876	.1899	.1832	.1964	.207	.2342
% Δ from baseline	-20.021	-23.989	-15.987	-17.368	-17.035	-15.864	-10.568	-12.078	-6.79
Municipalities	512	1023	1534	2045	2556	3068	3579	4090	4601
Observations	1,200,128	2,397,912	3,595,696	4,793,480	5,991,264	7,191,392	8,389,176	9,586,960	10,784,744
(B) <i>Hours 0/1</i>	10th pct (1)	20th pct (2)	30th pct (3)	40th pct (4)	50th pct (5)	60th pct (6)	70th pct (7)	80th pct (8)	90th pct (9)
DST	-0.0097 (0.007)	-0.0093** (0.004)	-0.0074** (0.004)	-0.0085*** (0.003)	-0.0101*** (0.003)	-0.0089*** (0.002)	-0.0081*** (0.002)	-0.0092*** (0.002)	-0.0097*** (0.002)
Municipality F.E	YES	YES	YES	YES	YES	YES	YES	YES	YES
Day-of-week F.E	YES	YES	YES	YES	YES	YES	YES	YES	YES
Day-of-month F.E	YES	YES	YES	YES	YES	YES	YES	YES	YES
Election	YES	YES	YES	YES	YES	YES	YES	YES	YES
Carnival	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year	YES	YES	YES	YES	YES	YES	YES	YES	YES
Municipal trend	YES	YES	YES	YES	YES	YES	YES	YES	YES
Baseline mean	.0305	.0253	.0268	.0266	.0269	.0255	.0268	.0274	.0301
% Δ from baseline	-31.721	-36.922	-27.597	-31.767	-37.374	-34.928	-30.286	-33.461	-32.07
Municipalities	512	1023	1534	2045	2556	3068	3579	4090	4601
Observations	1,200,128	2,397,912	3,595,696	4,793,480	5,991,264	7,191,392	8,389,176	9,586,960	10,784,744

Note: This table presents difference-in-differences estimates of the effect of DST on homicide rate per million inhabitants while restricting the sample of Table 4 to share-of-urban-population-percentiles, from 33.9% up to 93.2%. The panel (A) *Whole day* presents estimates considering all hours of the day, whereas panel (B) *Hours 0/1* restricts the sample to sunset hours. Row *Baseline mean* displays the mean of the outcome for treated units during the pre-treatment period in question. Row % Δ from baseline displays the percent variation from $\hat{\beta}_1$ with respect to the baseline mean. Specifications include municipality fixed effects, day-of-week fixed effects, day-of-month fixed effects, year fixed effects and, when applicable, municipal-specific linear trends. Standard errors clustered at the state level are presented in parentheses. ***, **, and * represent $p < 1\%$, $p < 5\%$ and $p < 10\%$ respectively.

A Appendix

A.1 Metadata

Death from external causes

Brazilian microdata on deaths caused by an external event were downloaded from SUS's website at <http://www.datasus.gov.br/DATASUS/index.php?area=0901&item=1&acao=26> (Last download on September 3 2017). At the time of the last download, hours for the daily records of death certificates were available only from 2006 to 2015, which is the period included in our analysis. To avoid discretionary editing of the data, we abstain from recoding any entry for which the hour of death is not recorded in the standard HH:MM format. Examples range from easily distinguishable cases (e.g 18H15, 19?30, 22/00) to far from obvious entries (e.g, 01-1, 0173, 01;0). Codebook for the original data can be found at ftp://ftp.datasus.gov.br/dissemin/publicos/SIM/CID10/Docs/Estrutura_SIM_para_CD.pdf (Last download on September 3 2017). A convenient application for browsing descriptions of each ICD-10 code used in our classification can be found at <http://apps.who.int/classifications/icd10/browse/2010/en>, under "XX External causes of morbidity and mortality" (last accessed Nov 3 2017).

Sunlight hours

Sunlight hours for each location-date in the period analyzed were calculated using the function `getSunlightTimes()`, of the R package `suncalc` version 0.4 running on R version 3.4.3 (for more information, please refer to <https://cran.r-project.org/package=suncalc> - Last accessed on December 16 2018). Sunset hour is defined as the hour the "sun disappears below the horizon, evening civil twilight starts". In our regressions, the hour of death relative to sunset time is the intra-date difference between the recorded hour of death and the sunset hour at any given municipality-date. Non-integer relative hours of death are then rounded down. Take the example

of the relative sunset hour, which assumes value 0 at the exact time the sun disappears below the horizon; any value in the interval $[-1; 0)$ means that the death occurred during the hour before the the sun disappears below the horizon, thus this death goes into the hour -1 . On the other hand, any value in the interval $[0; 1)$ belongs to the relative hour 0, which covers the hour following the disappearance of the sun below the horizon.

Municipal characteristics

Municipal area and distances to border were calculated using official GIS data provided by the national bureau of statistics (IBGE). Those were computed over the Brazilian 2007 municipal shapefile 55mu2500psr, which can be found at https://downloads.ibge.gov.br/downloads_geociencias.htm#. Urban population, total population, and GDP come from the last National Census, in 2010, also carried by IBGE. This information is conveniently available at <http://www.ipeadata.gov.br>.

A.2 Tables

Table A1: Descriptive statistics: Individual (deceased person) and municipal characteristics

<i>(A) Individual</i>	Treated (1)	Control (2)	Difference (3)
Black	.090 (.2857)	.076 (.2645)	.014*** (.0011)
Age	29.462 (11.750)	28.687 (11.582)	.775*** (.045)
Married	.137 (.3442)	.122 (.3271)	.015*** (.001)
Years of education	6.119 (2.928)	5.022 (2.988)	1.098*** (.013)
Observations	129,738	139,090	N = 268,828
<i>(B) Municipality</i>	Treated (1)	Control (2)	Difference (3)
per capita GDP (×1,000)	16.951 (15.280)	6.734 (8.731)	10.217*** (.365)
Population density	142.345 (709.202)	75.794 (368.425)	66.551*** 16.674
% Urban population	.7084 (.2140)	.5583 (.1968)	.1501*** (.0058)
Population (×1,000)	40.868 (261.594)	31.606 (112.950)	9.262 (5.990)
Area Km ² (×1,000)	1.025 (2.439)	2.506 (8.559)	-1.481*** (166.239)
Observations	2,943	2,169	N = 5,112

Note: This table presents descriptive statistics, decomposed by treatment status, on characteristics of municipalities and deceased victims. Information on victims are available in the SIM database while municipal characteristics were retrieved from the 2010 Brazilian census, which is the last census available.

Table A2: Difference-in-differences estimates: heterogeneity by percentile of distance-bandwidths around state borders for +- 30 days around transition date

(A) <i>Whole day</i>	10th pct (1)	20th pct (2)	30th pct (3)	40th pct (4)	50th pct (5)	60th pct (6)	70th pct (7)	80th pct (8)	90th pct (9)
DST	-0.0721* (0.037)	-0.0486** (0.021)	-0.0263 (0.018)	-0.0285** (0.014)	-0.0381*** (0.013)	-0.0428*** (0.012)	-0.0313*** (0.010)	-0.0293*** (0.009)	-0.0305*** (0.009)
Municipality F.E	YES	YES	YES	YES	YES	YES	YES	YES	YES
Day-of-week F.E	YES	YES	YES	YES	YES	YES	YES	YES	YES
Day-of-month F.E	YES	YES	YES	YES	YES	YES	YES	YES	YES
Election	YES	YES	YES	YES	YES	YES	YES	YES	YES
Carnival	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year	YES	YES	YES	YES	YES	YES	YES	YES	YES
Municipal trend	NO	NO	NO	NO	NO	NO	NO	NO	NO
Baseline mean	.4727	.4118	.418	.4073	.3443	.3228	.2789	.2787	.3017
% Δ from baseline	-15.259	-11.792	-6.282	-7.005	-11.06	-13.266	-11.211	-10.512	-10.1
Municipalities	1131	2028	2780	3248	3565	3817	3999	4200	4618
Observations	321,000	612,900	919,320	1,226,220	1,533,300	1,840,560	2,147,580	2,454,300	2,761,080
(B) <i>Hours 0/1</i>	10th pct (1)	20th pct (2)	30th pct (3)	40th pct (4)	50th pct (5)	60th pct (6)	70th pct (7)	80th pct (8)	90th pct (9)
DST	-0.0364*** (0.011)	-0.0177** (0.007)	-0.0153*** (0.005)	-0.0160*** (0.005)	-0.0152*** (0.004)	-0.0151*** (0.003)	-0.0137*** (0.003)	-0.0130*** (0.003)	-0.0139*** (0.003)
Municipality F.E	YES	YES	YES	YES	YES	YES	YES	YES	YES
Day-of-week F.E	YES	YES	YES	YES	YES	YES	YES	YES	YES
Day-of-month F.E	YES	YES	YES	YES	YES	YES	YES	YES	YES
Election	YES	YES	YES	YES	YES	YES	YES	YES	YES
Carnival	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year	YES	YES	YES	YES	YES	YES	YES	YES	YES
Municipal trend	NO	NO	NO	NO	NO	NO	NO	NO	NO
Baseline mean	.0662	.0551	.0521	.0474	.0393	.037	.032	.0317	.0354
% Δ from baseline	-54.935	-32.125	-29.369	-33.731	-38.763	-40.721	-42.947	-40.899	-39.373
Municipalities	1131	2028	2780	3248	3565	3817	3999	4200	4618
Observations	321,000	612,900	919,320	1,226,160	1,532,820	1,840,020	2,146,920	2,453,820	2,761,080

Note: This table presents difference-in-differences estimates of the effect of DST on homicide rate per million inhabitants while restricting the sample of Table 4 to +- 30 days around transition date and distance-percentiles around the border between treated and untreated states, from 190 km up to 1770 km. The panel (A) *Whole day* presents estimates considering all hours of the day, whereas panel (B) *Hours 0/1* restricts the sample to sunset hours. Row *Baseline mean* displays the mean of the outcome for treated units during the pre-treatment period in question. Row % Δ from *baseline* displays the percent variation from $\hat{\beta}_1$ with respect to the baseline mean. Specifications include municipality fixed effects, day-of-week fixed effects, day-of-month fixed effects, and year fixed effects. Standard errors clustered at the state level are presented in parentheses. ***, **, and * represent $p < 1\%$, $p < 5\%$ and $p < 10\%$ respectively.

SECOND CHAPTER
EDUCATED CANDIDATES AND EFFICIENT BUREAUCRATS

2.1 Introduction

Among individual characteristics that potentially play a role in politicians' performance in office, few have been as discussed in the literature as education attainment, being a stylized fact in economics and political science to proxy politicians' quality from levels of formal schooling (see Ferraz and Finan, 2009; Brollo et al., 2013; Atkinson, Rogers and Olfert, 2016, to mention only a few).

While there are compelling theoretical arguments for why more educated candidates would perform better in office across different measures, empirical studies hardly agree on the validity of this assumption, often pointing minute observed differences in outcomes, if any. For instance, regarding the impact of electing a more educated leader on education outcomes itself, while cross-country exercises suggest that educational attainment of the population is affected (Diaz-Serrano and Pérez, 2013), within-country analysis finds no difference whatsoever (Lahoti and Sahoo, 2016). Such discordance between increased welfare and no effect at all is also found with respect to fiscal policy (Rocha, Orellano and Bugarin, 2016; Freier and Thomasius, 2015), economic growth, and legislative productivity (Besley, Montalvo and Reynal-Querol, 2011; Ferraz and Finan, 2009; Carnes and Lupu, 2016).

A less touched strand on politicians' quality, however, regards whether more educated candidates make less corrupt politicians and more efficient managers of public resources.¹ Although scarce literature exploiting similar settings has found politicians with college degrees are not less likely to be corrupt (Carnes and Lupu, 2016), theoretical predictions over this outcome point to plausible opposite potential directions. For instance, under certain conditions, greater career concern on the behalf of the educated would imply higher risk aversion and lesser corrupt behavior (Brollo et al., 2013). On the other hand, as a reflex of educated politicians having higher potential gains in the private market, they may compensate wage differences through rent ex-

¹ For the sake of concision, throughout the paper I use more educated candidate, educated candidate, and college candidate interchangeably, meaning individuals with higher levels of education attainment than a baseline group.

traction (Caselli and Morelli, 2004). What the literature does not consider, however, is third possibility regarding no differences in deliberate corrupt behavior, but lower levels of ability to comply with managerial best-practices.

The aim of this paper is to test the hypothesis that educated candidates are more competent as public funds managers and make into less corrupt politicians. Earlier empirical evidence on that link would suffer from measurement error, since most studies were based on indices that measure perceptions rather than actual political corruption (Olken, 2009; Truex, 2011). Moreover, many have relied primarily on cross-country analysis, where the inability to account for the full set of institutional arrangements that determine both corruption and education has made results difficult to interpret. In many cases, even within-country analysis can be misleading, specially when one does not account for the initial choice-set of competing candidates, regarding simultaneously the distribution of education attainment among candidates and electoral competitiveness, which may be endogenously determined (Carnes and Lupu, 2016).

To circumvent the first caveat of measuring corruption, I take advantage of outcomes from a randomized anti-corruption carried by Controladoria Geral da União (hereafter, CGU), which makes observation of corruption levels independent of potential corruption, efficiency of local institutions, politicians' education attainment, voters' preferences towards educated candidates, and other pre-existent characteristics. Regarding endogenous selection of educated candidates into office, when directly comparing groups of municipalities that elected leaders with different levels of education, naive approaches do not take into account whether it results from voters revealed preference, as it might be that none of the candidates in a given municipality attained higher education. On the other hand, a municipal election in which the two most competitive candidates have a college degree is very likely to reflect a more educated electorate - or higher ability to attract votes being correlated with education attainment. Even under the exact same proportion of educated candidates in both groups of municipalities, a naive difference is still vulnerable to capturing different levels of

state capacity, which may correlate both with political preferences for more educated candidates and lower levels of pre-existing corruption.

Thus, using close-race elections regression discontinuity design (RDD) for municipalities where the elected candidate has a college degree and the runner-up has lesser education attainment and vice versa, Brazilian political structure lends municipal administrations to an empirical approach in which the choice-set of elected mayors is compatible with revealed preferences, and for which constituencies are balanced across institutional settings. In a framework where mismanagement is deeply ingrained, I find that candidates with higher formal education show no difference in likelihood of committing irregularities, while in the intensive margin more educated candidates commit from 8 to 11% less irregularities, a result sharply driven by a difference in the number of moderate-type infringements (from 9 to 13%). On the other hand, no difference is found regarding formal or severe infringements.

Further results are based on a subsample of CGU inspections in which auditors inform by how much mayors should refund the public administration for wasted or diverted funds. Surprisingly I find no significant difference between these two types of candidates either for having received a recommendation of refund or for the amount claimed for refund.² Taken together, those results suggest that this difference in compliance is likely due to lack of knowledge on administrative protocols (in contrast with the different levels education translating into different corrupt behavior), since there is no monetary rent-extraction or, as I show, future electoral gains.

Nevertheless, this difference in ability to comply with public accounts and protocols seems to have consequences to municipal budget in the subsequent term, which is in line with previous literature (Brollo, 2008): reduced form estimation shows that, regardless of reelection, mayors in the subsequent mandate where a college candidate was elected receive roughly 60% more discretionary transfers from higher offices (Federal and State governments). As a last exercise, I provide regression results for

²The amount claimed for refund is based on the contracted value paid by the mayor for the goods and services in question, the market value of those goods and services, and whether the totality of those goods and services were delivered. Throughout the paper I use this as a proxy for the amount of funds diverted or spent without proper substantiation

alternative outcomes of efficiency such as administrative expenditures and relative size of public staff, while ruling out electoral strategic behavior as drivers. College candidates spend 7.6% less than non-college candidates with administrative functions and inflate their temporary staff by 32.8 to 36.8% less than non-college candidates. With no differences in reelection aspirations and reelection likelihood between college and non-college candidates, I take these last results as further support of difference in efficiency instead of difference in rent-extraction. In a framework of voters' choice this interpretation implies that citizens can validly process candidates' education attainment as a relevant signal of improved potential managerial outcomes, although education itself comes attached to other unobservable characteristics.

The remainder of this paper is structured as follows. Section 2.2 provides a picture of Brazilian local elections and administration and of the randomized program exploited for corruption and mismanagement measures while discussing the data sources used. Section 2.3 discusses the empirical strategy and presents supporting evidence to its internal validity. Finally section 2.4 presents the findings and discusses results, while concluding remarks are presented in section 2.5.

2.2 Background and data

Local Governments and Municipal Elections

Brazilian municipalities enjoy the status of federation members, not being subordinated to its State or Federal governments. This strong decentralization lends municipal administrations, in the figure of the local executive office, to be the main actors in local expenditures, policy implementation, and provision of basic health care, primary education, and infrastructure. Nevertheless, Brazil's tributary structure concentrates taxes collection at the Federal and State levels, with municipal taxes representing only 6% of total municipal revenue (Brollo and Troiano, 2016). Federal and State administrations then distribute tax revenue across municipal administrations over two mechanisms: Automatically, through mandatory constitutional transfers, or upon

application, through discretionary transfers called *convênios*.

The representative of municipal administrations are the mayors, head of the executive office, which are democratically elected every four years, and eligible for reelection for one subsequent mandate. Voting is compulsory for citizens aged 18-70 and enforced by legal sanctions if not justified, which supports a turnout rate around 87%. In this framework of compulsory voting, common sources of self-selection of voters such as pro-sociality, and social pressure should play virtually no role in driving who ends up getting elected because of specific types of voters who turnout (Hortala-Vallve and Esteve-Volart, 2011).

Information on Brazilian municipal elections for 2005-2008 and 2009-2012 are publicly provided by the Superior Electoral Court (TSE), and include candidates characteristics - such as education attainment, gender, political affiliation, occupation - and electoral outcomes - such as valid votes, turnout, number of candidates competing, and votes per candidate. I use the latter variable to compute the margin of victory by which the elected candidate won over the second-place candidate (also called runner-up candidate). Electoral outcomes for municipalities are then linked to those for national elections, which also happen every four years, between municipal elections. This allows me to account for political alignment between mayors and the federal government, which is responsible for most of the transfers.

Data on local governance and additional municipal characteristics come from various sources. Detailed information on municipal finances are made publicly available by the Treasury Secretariat (FINBRA). It includes revenues and spending disaggregated by topic, allowing the construction of relevant municipal budget variables such as transfers received from higher offices and administrative expenditures. The Brazilian Municipalities Profile Survey, conducted by the National Bureau of Geography and Statistics (IBGE), provides information on the size and composition of the local government, and on the presence of relevant infrastructure such as radio and tv broadcasters, and the existence of higher education institutions in the municipality, whereas demographic characteristics are retrieved from 2000-2010 national census (IBGE).

Randomized Anti-Corruption Program

In response to the lack of state capacity to detect malfeasance and the systemic corruption that became evident following the transition to democracy,³ the federal government created in 2003 the Controladoria Geral da União (CGU), a ministry that centralizes the government's internal control and sets directives for promotion of efficient use of public resources. In that same year CGU launched a program consisting of random audits of municipalities for their proper use of federal transfers, which represent a fundamental component of municipal budgets to support the public provision of basic services and goods.

In each round of the program, which has audited more than 2,000 municipalities with over R\$22 billion worth of federal funds, municipalities are randomly chosen for inspection through publicly held lotteries. Once municipalities and inspection topics are drafted, CGU sends a team of auditors to cross-check accounts and documents, inspect the delivery of public services, and verify the existence and quality of infrastructure to which the federal transfers were intended (Avis, Ferraz and Finan, 2017). Shortly after the information from field inspections are reported to the central office, detailed reports with the results for each inspected municipality are published on-line and sent to the competent authorities, including federal courts and the local judiciary, with visible repercussion in the media and to federal, state, and local authorities (refer to Ferraz and Finan, 2008, 2011; Litschig and Zamboni, 2016; Avis, Ferraz and Finan, 2017; Muço, 2017, for specific examples).⁴

From 2006, on the 20th round of the program, the audit reports started to be compiled internally by CGU into readable data containing the dates of the lotteries and report release, the area to which the fund should have been invested, the federal government program to which the transfer was attached, the amount transferred, and the quantity of irregularities found, classified as formal/procedural, moderate, or

³Brazil experienced a military regime from April 1 1964 to March 15 1985, returning to full democracy three years later with the passing of the 1988 Constitution.

⁴ Reports can be accessed and downloaded through CGU's website, under *Fiscalização em Entes Federais - Municípios > Fiscalização Sorteio de Municípios* (Retrieved on July 11, 2017)

severe, according to degree of materiality and compromise to the program, based on CGU's internal normative. In this analysis, I focus on irregularities committed during the 2005-2008 and 2009-2012 electoral terms. Hence, the main estimation sample consists of all audits conducted from March 2006.⁵

Defining “Educated” candidates

The definition of “educated” in the literature has been synonymous of having a college degree especially because most electoral data do not provide years of schooling but education level (e.g., primary school, high school, and college). I opt for defining an educated candidate as a candidate having at least a college degree for two main reasons. First, the recent political debate in Brazil regarded the possibility of requiring candidates to have a college degree in order to be eligible.⁶ Second, almost half of the candidates over the electoral universe have a college degree, making this group a favorable counterpart. Additionally, this definition is a standard in the literature in the absence of a non-categorical variable such as years of schooling. Thus, in the present exercise being an educated candidate means having at least a college degree, to which I refer as college candidate, in contrast with having a lesser education attainment, to which I refer as non-college candidate. To enforce all municipalities in the sample have the same choice-set regarding candidates' education, the sample is restricted to elections for which ($1^{st} place_{college}=1$; $2^{nd} place_{college}=0$) or ($1^{st} place_{college}=0$; $2^{nd} place_{college}=1$), to which I refer as *college-races*.

Then, the last data-processing step regards defining the treatment assignment mechanism. The key feature of any RD design, discussed in details in section 2.3, is the presence of a running variable with a known threshold that determines treatment

⁵ The analysis would be compromised in case characteristics correlated with mayors' education attainment are also correlated with how thoroughly CGU auditors investigate and report irregularities. Nevertheless, CGU is seemed as one of the government's most autonomous and least politicized agencies. Regarding concerns that CGU auditors may themselves be corrupt, previous works failed to find any evidence that the auditors manipulate their reports (Hidalgo, Canello and Lima-de Oliveira, 2016; Bersch, Praça and Taylor, 2017).

⁶ The Constitutional Amendment Proposal (PEC) n.119/2015 proposes that candidates to legislative and executive offices should have at least a college degree in order to be eligible. This PEC was deemed admissible on May 12 2016 by the congress Commission for Constitution, Justice, and Citizenship, and waits for further appreciation before being voted.

assignment for each unit in the sample. In the present case, which exploits a sharp change in the probability of being treated, units with a value of the running variable greater than or equal to the threshold receive the treatment, which is electing a college mayor, whereas units with a value of the running variable lesser than the threshold do not receive the treatment, which means electing a non-college mayor. In the case of close-race elections, this running variable is the margin of victory (MV) by which the elected candidate won over the runner-up, and the threshold that divides treatment and control groups is $MV = 0$: To ensure that all non-college candidates are on one side of the cutoff (left side) and all college candidates are on the other (right side), MV is transformed following the scheme below.

$$MV = \begin{cases} vote\ share_{college=1} - vote\ share_{college=0}, & \text{if } 1^{st} place_{college=1} \\ (vote\ share_{college=0} - vote\ share_{college=1}) \times (-1), & \text{if } 1^{st} place_{college=0} \end{cases}$$

Descriptive statistics

The final sample of audited municipalities for which the elected candidate has different education attainment from the runner-up, with one of them having at least a college degree, is shown in figure 1. Municipalities where a college candidate was elected are represented by blue diamonds whereas those where a non-college candidate won are represented by red circles. It is fair to say that there is no meaningful discrepancy in the geographic distribution of those two groups across the country.

[Figure 1 about here.]

The composition of this non-college group is explicit in figure 2. The upper panel shows the education distribution for all elected candidates (top left) and all non-elected candidates (top right) over the 2005-2008 and 2009-2012 electoral terms. The lower panel shows the education distribution for elected candidates (bottom left) and runner-up candidates (bottom right) only over college-races, suggesting that the final sample is not abnormally different from the electoral universe in terms of the distribution of

education among candidates.

[Figure 2 about here.]

For the overall final sample as well as disaggregated by treatment status, table 1 presents descriptive statistics over the considered variables, which are divided in four panels: (A) Audits and (B) Bureaucracy, comprising the main outcomes, and (C) Elections and (D) Municipality, comprising the covariates used to provide illustration of the balance between treatment and control groups.

Although most variables are self explanatory, some need further consideration. For instance, in the panel (A), which lists the variables retrieved from CGU's dataset, Health, Education, and Social assistance program are indicator variables equal to one if the funds audited were intended to Health, Education, or Social assistance. Programs involving other areas such as transportation and agriculture do exist, however, they are not present through all rounds of inspection or all municipalities. This way, I focus on Health, Education, and Social assistance, which account for 73.28% of issues audited (also referred to as inspection orders), 78% of inspected funds, and concentrate 80.65% of all irregularities found by the program.

[Table 1 about here.]

In panel (B), besides municipal expenses, two other outcomes are # Public servants pc, the per-capita number of administrative employees working directly to the executive office, and # Commissioned pc, the per-capita number of temporary (mandate-dependent) positions directly appointed by the mayor without clear predefined criteria or requirements for both admission and dismissal. In contrast with tenured or permanent employees, who must pass a civil service entrance exam and cannot be easily dismissed by the mayor due to de jure job stability, temporary positions (*cargos comissionados*) may proxy engagement in political patronage to gain reelection, since it ties the continuation utility of a voter to the political success of the politician in charge (Brollo and Troiano, 2016; Colonnelli, Teso and Prem, 2018). Those variables, jointly

with data on reelection success, are used to provide insights on rent-extraction versus difference-in-efficiency hypotheses.

2.3 Empirical Strategy

Education attainment is strongly correlated with social and family background plus a number of other individual-specific characteristics, which may include organizational skills, diligence, social preferences, etc. Thus, even under randomized assignment of mayors with different levels of education, it would not be feasible to estimate the effect of leaders' *education itself* on governance and policy outcomes.⁷ It follows that providing relevant insights on whether more educated candidates perform better in office is equivalent to testing the validity of education attainment as a predictor of bureaucratic efficiency instead of its immediate cause. In a framework of voter's choice this interpretation still implies that citizens can validly process candidates' education attainment as a relevant signal of improved potential managerial outcomes, although education itself comes attached to other unobservable characteristics.⁸

Nevertheless, even though we recognize the endogenous nature of individuals' education attainment, simply estimating the difference in governance outcomes for the two groups would face several shortcomings. For instance, even under the exactly same proportion of educated candidates in both groups of municipalities, a naive difference still faces the possibility of capturing different levels of state capacity and welfare correlated with political selection of less corrupt candidates that happen to be more educated. Along the same lines, a naive difference may inflate results by capturing the effect of differences in political competition, and therefore post-election oversight, in case less educated candidates are elected in less competitive elections.

In the absence of tools able to account for the full set of social and institutional

⁷Although that procedure would eliminate endogenous political and electoral selection into office, it would not eliminate individuals' endogenous selection into different levels of formal education, which is a function of several other individual-specific features that may affect the way mayors manage municipalities.

⁸This is specially important in a framework of voters' rational choice based on policy positions and non-policy information where parties and platforms are highly volatile.

arrangements that determine both pre-treatment characteristics and observed outcomes, I rely on close elections regression discontinuity design to compare levels of non-compliance in the use of public funds between municipalities in which a more educated candidate barely won the election against a less educated runner-up (treatment group) to those where a less educated candidate barely won an election against a more educated runner-up (control group). Analytically, the difference in outcomes between municipalities that could have elected a non-college candidate but ended up barely electing a college candidate by a small advantage on the ballots and vice versa can be expressed as $\tau = \lim_{MV \downarrow 0} E[Y|MV] - \lim_{MV \uparrow 0} E[Y|MV]$, where Y is the level of outcome in question and MV is the margin by which the first placed candidate outperformed its runner-up, as each function approach an infinitesimal advantage towards zero. Since we don't know the true form of $E[Y|MV]$, the estimated parameter of interest takes the form $\hat{\tau} = \hat{\alpha}_1 - \hat{\alpha}_0$, where $\hat{\alpha}_1$ and $\hat{\alpha}_0$ are the intercepts as $MV \rightarrow 0$ on each side of the cutoff, obtained from the solution of the minimization problem in equation (1).

$$(1) \quad \begin{bmatrix} \hat{\alpha}_0 \\ \hat{\beta}_0 \\ \hat{\alpha}_1 \\ \hat{\beta}_1 \\ \boldsymbol{\gamma} \end{bmatrix} = \arg \min_{\alpha_0, \beta_0, \alpha_1, \beta_1, \boldsymbol{\gamma}} \sum \{Y_i - \mathbb{1}(MV_i < 0)(\alpha_0 + \beta_0 MV_i) - \mathbb{1}(MV_i > 0)(\alpha_1 + \beta_1 MV_i) - \mathbf{Z}_i \boldsymbol{\gamma}\}^2 K_{tri}(MV_i)$$

In which \mathbf{Z}_i is a vector of balanced covariates aimed to improve efficiency (Calonico et al., 2017) and $K_{tri}(MV_i)$ is the triangular kernel function of MV , which serves to localize the regression fit near the cutoff by attributing greater weight to observations the closer they are to $MV = 0$.

Nevertheless, although RDD imposes less restrictive requirements for identification than other observational tools, its internal validity is based on the assumption

of continuity of potential outcome in the absence of treatment. Given the exclusive nature of counterfactual analysis, however, this assumption is not directly testable. In turn, standard practice involves providing results for two empirical exercises as follows.

To rule out endogenous electoral selection into treatment, which could be caused, for instance, by voters' strong preferences for college candidates or by higher ability to attract votes being correlated with education attainment, a standard practice is to show the absence of a density discontinuity around the cutoff. The underlying assumption here is that, if having at least a college degree does not predict the margin of victory of college candidates, then the number of treated observations just above the cutoff should be similar to the number of control observations below it. Therefore, in table 2 I provide nonparametric estimates to test the density continuity around the cutoff, which are computed based on local polynomial techniques recently proposed by Cattaneo, Jansson and Ma (2017). As required, the tests do not suggest any significant difference in density across $MV = 0$. Additionally, the widely used McCrary's discontinuity test, which estimates the log difference in height of the empirical density distributions around the threshold, is not significant as well – $-.036$ (.166).

[Table 2 about here.]

To reinforce the evidence suggested by the aforementioned tests, as well as to rule out that endogenous institutional characteristics are the actual drivers of the estimated effect, the second standard practice is to show that relevant baseline characteristics are not discontinuous around the cutoff. Although this does not imply that the potential outcome would be continuous around the cutoff in the absence of treatment, discontinuity in pretreatment covariates relevant for the outcome of interest may suggest that institutional arrangements are pushing the types of would-be barely-losers up above the threshold or moving potential barely-winners down below the threshold. For instance, a non-college candidate that runs in close election against a college candidate and wins may be better-off in other dimensions such as political alignment with higher offices, incumbency, or support from client voters that benefit

from his party holding office. Therefore, in figure 3 I provide estimated discontinuities, along with conservative 90% level confidence intervals, over baseline characteristics, which are estimated in the same fashion of equation 1 except for the absence of controls, following de la Cuesta and Imai (2016). It is clear that every considered baseline variables related to the inspection program itself, government characteristics in the beginning of the mandate, municipal features, and most electoral characteristics are balanced.

[Figure 3 about here.]

Moreover, the borderline differences are solely driven by individual-specific features: Having previous political experience as an executive or legislative elected official, being a female, and being affiliated to PSDB (Brazilian Social Democracy Party). As education attainment is a function of individual tastes and restrictions, which are naturally expected to be correlated with other individual self-selected characteristics, the only consequence of such imbalance is that one cannot interpret the coefficient τ as the effect of education itself, as already acknowledged. Since the goal in this paper is testing the validity of education attainment as a signal of bureaucratic efficiency instead of its immediate cause, this minimal imbalance in terms of point estimates can be seen as a direct imbalance at the candidate level instead of in institutional variables able to affect both selection into treatment and potential corruption levels. Nevertheless, given the relationship between political experience, gender, and efficiency (Brollo and Troiano, 2016; Coviello and Gagliarducci, 2017), I provide results over restricted samples to show the effect is not driven by each of these characteristics.⁹

⁹Despite widely spread misconception regarding the necessary conditions for RD design to provide internally-valid treatment estimates, the presence of randomized characteristics is not necessary - although it would be sufficient. In fact, even if there is imbalance in pretreatment covariates just below and above the threshold it does not necessarily imply the violation of the identification assumption for the RD design as long as there is no discontinuous jump at the threshold (Snyder, Folke and Hirano, 2015; de la Cuesta and Imai, 2016).

2.4 Results

Figure 4 displays the main findings graphically for the log number of irregularities per issues audited in college-races: non-college candidates that won over college candidates to the left of $MV = 0$ and college candidates that won over non-college candidates to the right of $MV = 0$. In the top panel we see great dispersion in the level of procedural irregularities found, with no clear difference between the two groups, whereas for Moderate charges there is a clear and highly significant difference between the two groups at the cutoff. In the bottom panel are displayed analogous graphs for Severe and Total charges, the latter being the sum of Formal, Moderate and Severe. Jointly, we see that the difference in Total charges is driven by Moderate charges.

[Figure 4 about here.]

Regression results for the logarithm of charges per issue audited as the dependent variable are presented in table 3, preceded by estimates of the likelihood of having committed at least one of each type of infringement. Results for the dummy variables, although not significant point a trend of college candidates being less likely to commit formal or moderate infringements, but more likely to commit severe ones or any infringement at all, although for the latter point estimates are fairly close to zero. For the logarithm of charges, results agree between linear (top panel) e quadratic (bottom panel) specifications, pointing that college-educated candidates commit 9.7-13.1% less moderate infractions, which drives a reduction by 8-11% in total charges.^{10,11}

[Table 3 about here.]

¹⁰In exercises not reported but available upon request I estimate the same regressions restricted to narrower pre-selected intervals of MV following Calonico, Cattaneo and Titiunik (2014), which imposes windows sized from $MV = [-0.07, 0.07]$. The reduction sometimes by 50% in sample size (from 520 to 253 observations), however, gives point estimates that are not statistically significant, but suggest the same pattern as those obtained through an unrestricted range of MV . For instance, estimates for $\ln(\#Moderate)$ and $\ln(\#Total)$ are respectively -0.168 (0.126) and -0.092 (0.120) using local linear regression. Moreover, sample size for other outcomes analyzed later make local techniques unfeasible. That said, I refrain from using local estimation and rely on the full sample while applying the triangular kernel function on MV

¹¹Table A1 presents results for infringements per issue audited in level instead of in logarithmic transformation. In general, results are in accordance with table 3.

Given the borderline imbalance in political experience, gender, and being affiliated to PSDB (although $p\text{-value} > 0.10$), previously shown in figure 3, and the relationship between those variables and managerial outcomes found by previous literature (Brollo and Troiano, 2016; Coviello and Gagliarducci, 2017), I provide results in table 4 over restricted samples to show the effect is not driven by these individual-specific characteristics correlated with being a college candidate. Although there is no suggestion of imbalance in candidates facing different likelihood of being on their second subsequent mandate (i.e., facing a term limit), the first panel also presents results for a sample restricted to first-term mayors, following Ferraz and Finan (2011), who pointed that due to decreased electoral concerns second-term mayors tend to extract increased levels of rent.¹²

[Table 4 about here.]

Regarding the intensive margin of mismanagement, results in table 4 generally follow the same pattern exhibited by overall regressions in table 3, with college candidates committing less total infractions mainly through reduced levels of moderate infractions by 9.3-12.5%. Moreover, in exercises not reported but available upon request I estimate the same regressions restricting the samples according to pairwise combinations of the restrictions in table 4, as well as for all characteristics combined, for which results exhibit a similar pattern. Regarding that latter combination, no significant difference is found with respect to the dummies whereas estimates for $\ln(\#Formal)$, $\ln(\#Moderate)$, $\ln(\#Severe)$, and $\ln(\#Total)$ are, respectively: .023 (.043); -.226*** (.086); .014 (.075); -.142* (.082). Jointly those results suggest this rather small imbalance at the candidate level, which is likely to stem from a greater process of self-selection that also involves education attainment, is not driving the predictive power of candidates' education over proper use of public resources.

An immediate question that follows regards the difference in the amount of public funds wasted as consequence of the different levels of irregularities found. In trying

¹²In fact, restricting the sample to each of these characteristics may even increase imbalance in other correlated dimensions. For each of these restricted samples, however, I obtain estimates in the fashion of Figure 3 to learn that no further imbalance was created. Results are available upon request.

to translate these differences in efficiency into monetary costs, previous studies relied on the ratio of the transfer in which an irregularity was found divided by the total audited amount of transfers. Nevertheless, the numerator of this ratio is defined as the whole amount transferred to that municipality by the given program in contrast to the amount corrupted relative to the total amount transferred. For instance, even if two municipalities receive the same number of funds with each fund of the same amount, a municipality that engages in 25% over-invoice above market value when acquiring inputs would be pointed as inefficient as one that engages in 400% over-invoice based on the same budget. This fraction thus is not informative regarding the amount of money wasted.

Nevertheless, in an effort to enforce provision and quality of school meals in public schools, from lotteries 34 to 40 CGU not only inspected municipalities and reported irregularities, but also provided specific recommendations on the infringements involving funds of the National Program for School Feeding (Programa Nacional de Alimentação Escolar, hereafter PNAE). These sub-sample comprised 371 municipalities (6.67% of Brazilian municipalities), over 1,797 schools across the 26 states. The final PNAE audit findings reported how much with respect to the total PNAE transfer the mayor should return to public budget to compensate for the amount he or she wasted. Based on this dossier I can construct a relative measure of monetary waste compatible with malfeasance.¹³

[Table 5 about here.]

In table 5 I present regression results for which the dependent variable is dummy that indicates whether that mayor received any recommendation for refunding the program (columns 1 and 2) and for which the dependent variable is the ratio of the amount pointed by CGU auditors for refund divided by the original amount of the PNAE fund (columns 3 and 4). Parametric and non-parametric estimations show minute and not significant difference in recommendations by CGU, especially for the

¹³This report is available at CGU's website under Relatório de Avaliação da Execução de Programas de Governo Nº 63 - Apoio à Alimentação Escolar na Educação Básica. Refer to pages 75-92 for tabulated data. (Last accessed on October 26, 2017).

relative amount of funds diverted or spent without proper substantiation. Since both candidate-groups have the same propensity of being audited for the use of Education funds, as seen in figure 3 and verification regressions over irregularities restricted to Education funds show a similar pattern as that observed across table 3, difference in strategic corruption between college and non-college candidates regarding the nature of the program should not be a concern. Jointly, these results are in line with considerations previously made by CGU, suggesting that a considerable share of non-compliance (here, the levels of irregularities found) is likely due to lack of proper public-administration knowledge - in contrast with the less education more corruption hypothesis.¹⁴

This difference in ability to comply with public accounts and protocols, however, has consequences to municipal budget in the subsequent term. In line with previous literature, which points that the Federal government suspends discretionary funds to municipalities where irregularities were found (Brollo, 2008), a reduced form estimation shown in table 6 point that, regardless of reelection cases, mayors in the subsequent mandate where a college candidate was elected receive at least 60% more discretionary transfers from higher offices (Federal and State governments).

[Table 6 about here.]

Column 1 of table 6 represents overall discretionary inter-government transfers in the current mandate, called convênios, the ones received upon application and appreciation by the higher office, while columns 2 and 3 disaggregate the convênios into the amount transfered by the federal government and that by state governments during the current mandate. Columns 4-6 display results for variables following the same definition, but computed for the subsequent mandate, i.e. the next mandate

¹⁴CGU's website: "...In many cases the problem is caused by lack of knowledge and unpreparedness by the public manager, and not by bad-faith or misconduct", under the sub-heading *Irregularidades*; CGU's website: "...lack of knowledge is another issue that must be resolved. 'The audit courts are composed of highly-qualified staff that are recruited and trained to overlook public management. On the other side, public managers are not always qualified and at the same knowledge level for the activity she or he is going to perform.'"; G1 News: "Lack of knowledge is the main cause for irregularities committed by mayors in the state of Mato Grosso do Sul, according to the according to Controladoria-Geral da União's (CGU) superintendent, José Paulo Barbieri." (Retrieved on October 17, 2017).

after a college degree candidate was elected. Results for the current mandate suggest, although not significant, that college candidates have, on average, greater success in receiving discretionary transfers from both higher offices. This, however, would not be enough for the significant difference in success found for the subsequent mandate, in accordance with previous literature on the effect of irregularities and malfeasance disclosure.

As a last exercise, I provide regression results for alternative outcomes of efficiency in table 7. Column 1 displays results for \ln of administrative expenditures over total municipal budget as the dependent variable. It suggests that college candidates spend 7.6% less than non-college candidates with administrative activities necessary for the functioning of the municipal executive. This cost is partially driven by a 3.3% lower variation in municipal staff between the first the last years of administration. Although increase in the size of government in either nominal or real terms cannot be seen a priori as a negative measure of efficiency in public administration (Besley, 2006), I recall that both college and non-college administrations start their mandates with the same average size of staff, as seen in figure 3.

Moreover, for the overall sample and across most subsamples the difference in inflation of temporary staff directly appointed by the mayor between the first and last years of mandate, cargos comisionados, range from -32.8 to -36.8%. Commissioned positions, however, can be thought as a proxy for engagement in political patronage to gain reelection, since it ties the continuation utility of a voter to the political success of the politician who provided the position (Brollo and Troiano, 2016). Nevertheless, in contrast with the clientelism hypothesis, the last two columns of table 7 rule out strategic behavior regarding electoral prospects, with no differences in reelection aspirations and reelection likelihood between college and non-college candidates. Taken together, this backs our difference-in-efficiency hypothesis instead of the rent-extraction one.

[Table 7 about here.]

2.5 Concluding Remarks

Among municipalities balanced across institutional features, I show that in the extensive margin more educated candidates are as likely as others to commit an irregularity. The intensive margin however shows a different picture, with more educated candidates committing from 8 to 11% less irregularities, which is driven by a sharp difference in moderate infringements whereas no difference is found regarding formal and severe ones. However, since estimations show no difference in the amount of funds diverted or spent without proper substantiation, results may be interpreted as being in line with conclusions derived by the institution that undertakes the auditing program - but regardless of education - that a substantial share of irregularities is caused by lack of administrative knowledge. This difference in ability to comply with public accounts and protocols, however, has consequences to municipal budget in the subsequent term, pointing that mayors in the subsequent mandate where a college candidate was elected receive substantially more discretionary transfers from higher offices. Whether similar findings extend to the context of developed countries is left as an open question. Nevertheless, it is worth noting that the empirical strategy applied in this paper could be applied in other countries that already count with similar auditing programs.

Across other dimensions, previous literature has not agreed on the governance effects of electing officials with different education levels. Joint with the results presented in this paper, which do not seem to point in the direction that college is associated with less corruption, but instead increased efficiency, it is likely that provision of training such as by CGU's own Program for Strengthening of Public Management (Programa de Fortalecimento da Gestão Pública), which offers public servants and elected officials training on public management good practices, may promote efficiency without any loss of representativity. Unfortunately, given the self-selection into this program, it is not feasible to provide hard evidence in that direction. Moreover, those results should be taken with caution, as implementing a policy to restrict

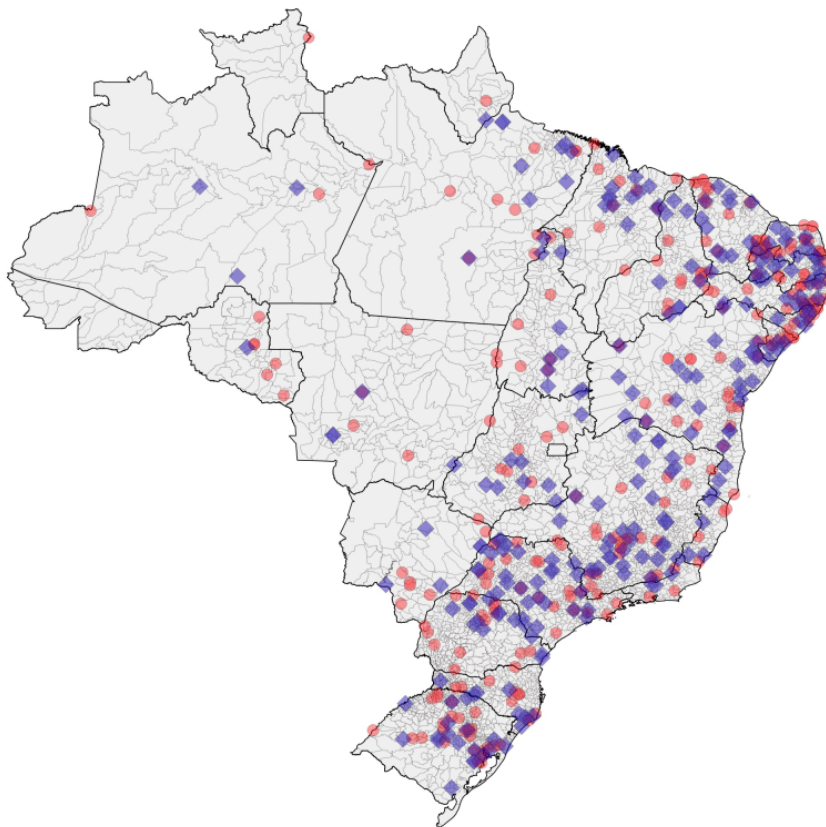
candidacy according to education attainment would result in a different general equilibrium of self-selection into politics and electoral selection, which, in the presence of barriers to the acquisition of education, may have undesirable consequences such as the undefined continuation of political dynasties.

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Figure 1: Final sample - college vs non-college elected mayors



Note: This figure displays the geographic distribution of each municipality in the final sample that elected, in a “college-race”, either a college candidate (blue diamonds) or non-college candidate (red circles). Superposed blue diamonds and red circles imply the same municipality enters the final sample for both mandates, 2005-2008 and 2009-2012.

Figure 2: Distribution of education: Population vs Selected sample

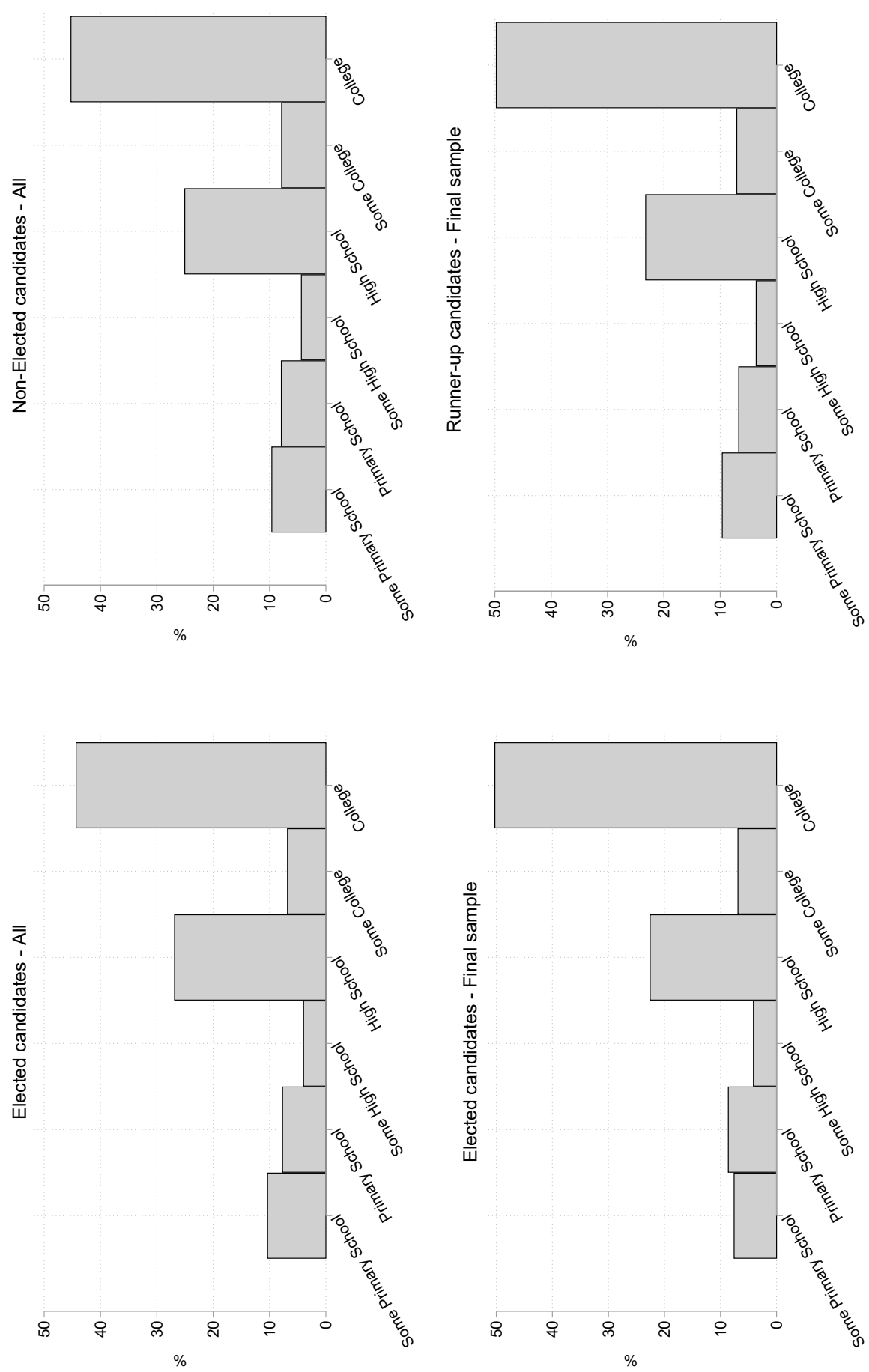
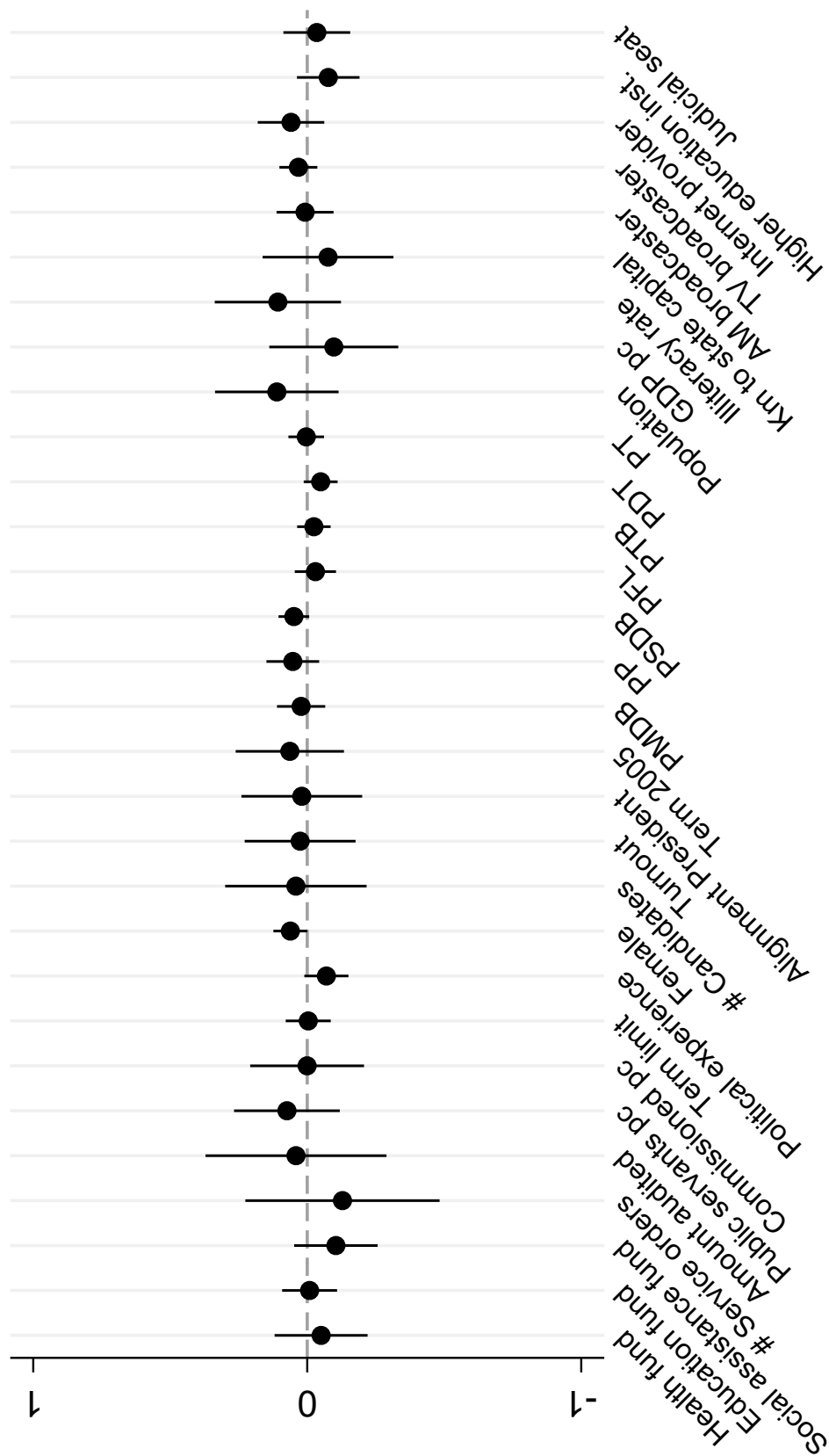
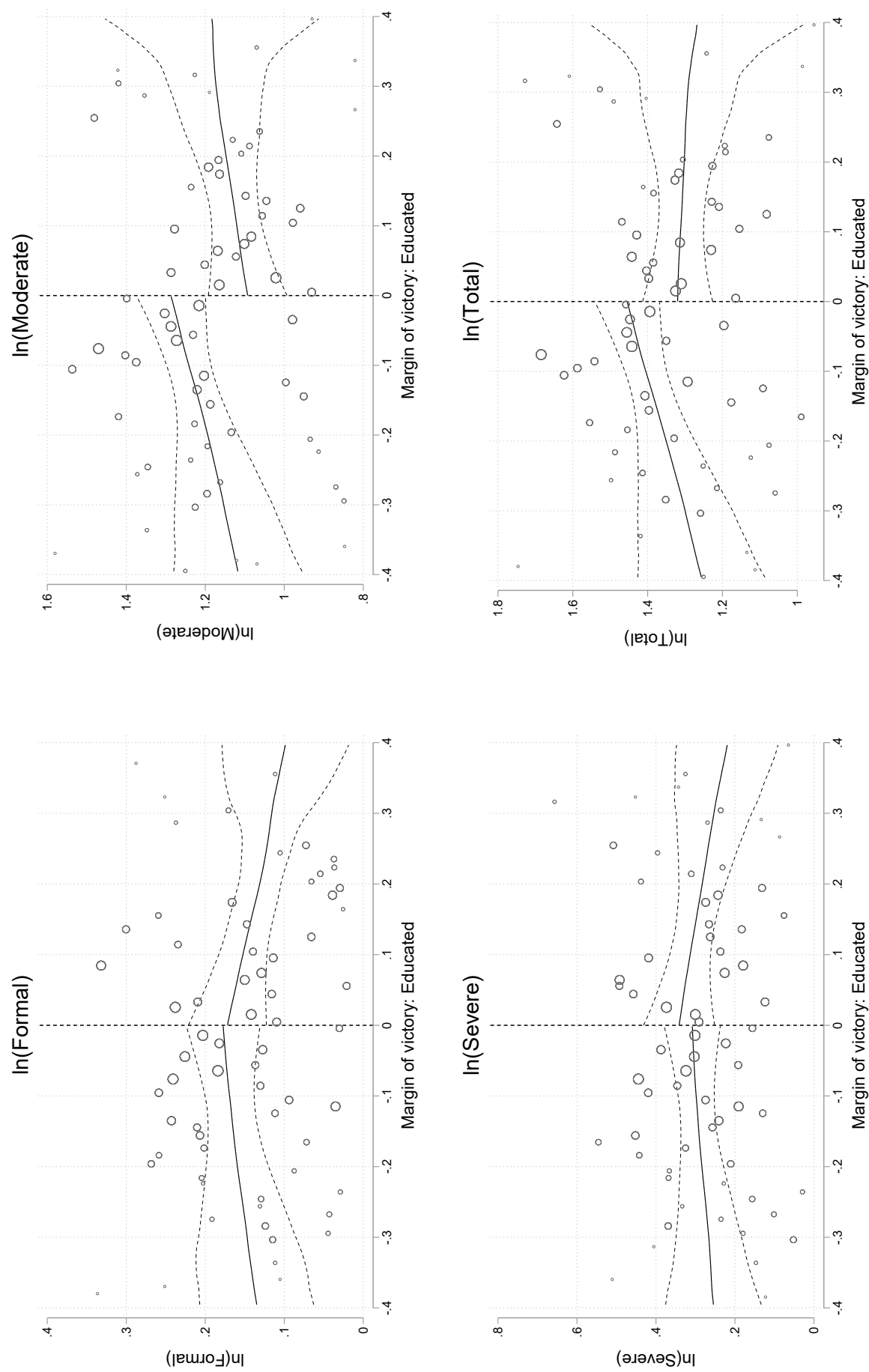


Figure 3: Estimated discontinuity on baseline characteristics with 90% level confidence intervals



Note: This figure plots the estimated discontinuity on baseline characteristics around the cutoff. For non-binary measures, dependent variables are transformed in standard deviation units, following de la Cuesta and Imai (2016). Dark vertical lines represent 90% level robust confidence intervals.

Figure 4: Graphical analysis: number of irregularities per issue audited averaged within 1 pp bins



Note: This figure plots the estimated discontinuity on irregularities around the cutoff - non-college candidates ($MV < 0$) vs. college candidates ($MV > 0$). Hollow circles represent the dependent variable averaged within 1 pp bins, with size proportional to the relative frequency of the latter.

Table 1: Summary statistics: Irregularities, Elections, and Municipal characteristics

Variables	Overall		college=0		college=1	
	mean	sd	mean	sd	mean	sd
<i>(A) Audits</i>	(1)	(2)	(3)	(4)	(5)	(6)
Any formal charge	0.567	(0.496)	0.589	(0.493)	0.545	(0.499)
Any moderate charge	0.943	(0.233)	0.962	(0.191)	0.922	(0.268)
Any severe charge	0.642	(0.480)	0.642	(0.480)	0.642	(0.480)
Any charge	0.964	(0.187)	0.974	(0.161)	0.953	(0.211)
Number of formal charges	2.193	(3.424)	2.355	(3.503)	2.027	(3.339)
Number of moderate charges	33.23	(27.41)	36.16	(28.72)	30.21	(25.71)
Number of severe charges	5.736	(9.202)	6.249	(9.782)	5.206	(8.549)
Total number of charges	41.16	(33.77)	44.76	(35.56)	37.44	(31.45)
Health program	0.716	(0.451)	0.725	(0.448)	0.708	(0.455)
Education program	0.939	(0.240)	0.943	(0.232)	0.934	(0.249)
Social assistance program	0.797	(0.403)	0.815	(0.389)	0.778	(0.416)
# Issues audited	12.16	(7.462)	12.76	(7.583)	11.55	(7.300)
ln Funds audited	14.29	(2.901)	14.24	(3.145)	14.34	(2.632)
<i>(B) Bureaucracy</i>	(1)	(2)	(3)	(4)	(5)	(6)
ln(Expenses/Budget)	4.582	(0.077)	4.578	(0.097)	4.586	(0.047)
ln(Adm expences/Budget)	2.708	(0.376)	2.740	(0.369)	2.676	(0.381)
# Public servants pc	0.043	(0.018)	0.043	(0.017)	0.044	(0.019)
# Commissioned pc	0.005	(0.005)	0.005	(0.004)	0.005	(0.005)
<i>(C) Elections</i>	(1)	(2)	(3)	(4)	(5)	(6)
MV	0.003	(0.196)	0.148	(0.129)	0.148	(0.127)
Term limit	0.351	(0.478)	0.343	(0.476)	0.358	(0.480)
Experienced politician	0.172	(0.378)	0.174	(0.379)	0.171	(0.377)
Female	0.078	(0.269)	0.06	(0.239)	0.097	(0.297)
# Candidates	2.774	(0.916)	2.823	(0.943)	2.724	(0.887)
Turnout	0.877	(0.057)	0.875	(0.056)	0.878	(0.057)
Alignment - President	0.132	(0.339)	0.121	(0.326)	0.144	(0.352)
Term 2005	0.544	(0.499)	0.558	(0.498)	0.529	(0.500)
PMDB	0.180	(0.385)	0.170	(0.376)	0.191	(0.394)
PP	0.125	(0.330)	0.098	(0.298)	0.152	(0.359)
PSDB	0.117	(0.322)	0.106	(0.308)	0.128	(0.335)
PFL	0.078	(0.269)	0.098	(0.298)	0.058	(0.235)
PTB	0.075	(0.263)	0.079	(0.271)	0.070	(0.256)
PDT	0.067	(0.250)	0.094	(0.293)	0.039	(0.194)
PT	0.065	(0.247)	0.053	(0.224)	0.078	(0.268)
<i>(D) Municipality</i>	(1)	(2)	(3)	(4)	(5)	(6)
Population	21,439	(33,778)	22,870	(38,159)	19,964	(28,567)
ln per capita GDP	8.614	(0.759)	8.645	(0.799)	8.582	(0.714)
Illiteracy rate	0.230	(0.123)	0.230	(0.126)	0.231	(0.120)
Km to state capital	248.5	(163.8)	252.2	(174.1)	244.6	(152.8)
1 ≤ AM radio broadcaster	0.203	(0.403)	0.204	(0.404)	0.202	(0.403)
1 ≤ Television broad.	0.113	(0.317)	0.121	(0.326)	0.105	(0.307)
1 ≤ Internet provider	0.515	(0.500)	0.513	(0.501)	0.518	(0.501)
1 ≤ college	0.341	(0.474)	0.370	(0.484)	0.311	(0.464)
Judicial seat	0.519	(0.500)	0.558	(0.498)	0.479	(0.501)
Observations	520		264		256	

Table 2: Manipulation Test using local density estimation

<i>Method</i>	Unrestricted		Restricted	
	T (1)	p-value (2)	T (3)	p-value (4)
Conventional	0.044	0.965	0.087	0.931
Undersmoothed	0.559	0.576	0.805	0.421
Robust Bias-Corrected	0.239	0.811	0.017	0.987
Bandwidth ($h_l; h_r$)	0.178	0.192	0.153	0.153
Effective N ($N_l; N_r$)	185	189	168	159

Note: This table presents nonparametric density continuity estimates around the cutoff, based on local polynomial techniques, as proposed in Cattaneo, Jansson and Ma (2017). All models used triangular kernel. Inference based on Jackknife VCE for “Unrestricted”, and Plugin VCE for “Restricted”.

Table 3: Irregularity indicators and log of the number of irregularities per issue audited

<i>(A) Linear</i>		$\mathbb{1}(\text{Formal})$ (1)	$\mathbb{1}(\text{Moderate})$ (2)	$\mathbb{1}(\text{Severe})$ (3)	$\mathbb{1}(\text{Any})$ (4)	$\ln(\text{Formal})$ (5)	$\ln(\text{Moderate})$ (6)	$\ln(\text{Severe})$ (7)	$\ln(\text{Total})$ (8)
Estimate		-0.053 (0.043)	-0.019 (0.019)	0.020 (0.055)	0.009 (0.015)	-0.011 (0.018)	-0.097** (0.044)	-0.004 (0.039)	-0.080* (0.042)
N_I		264	264	264	264	264	264	264	264
N_r		256	256	256	256	256	256	256	256
Obs		520	520	520	520	520	520	520	520
<i>(B) Quadratic</i>		$\mathbb{1}(\text{Formal})$ (1)	$\mathbb{1}(\text{Moderate})$ (2)	$\mathbb{1}(\text{Severe})$ (3)	$\mathbb{1}(\text{Any})$ (4)	$\ln(\text{Formal})$ (5)	$\ln(\text{Moderate})$ (6)	$\ln(\text{Severe})$ (7)	$\ln(\text{Total})$ (8)
Estimate		-0.044 (0.058)	-0.017 (0.028)	-0.015 (0.062)	0.018 (0.026)	0.004 (0.027)	-0.131** (0.063)	-0.005 (0.054)	-0.112* (0.062)
N_I		264	264	264	264	264	264	264	264
N_r		256	256	256	256	256	256	256	256
Obs		520	520	520	520	520	520	520	520

Note: This table reports the effect of electing a mayor with college degree on outcomes of mismanagement of public funds. The dependent variable in columns 1-4 is an indicator equals 1 if at least one irregularity in a given severity-category was found (or at least one irregularity of any category, in the case of Any charge). In columns 5-8 the dependent variable is the ln of the number of irregularities in a given severity-category per issue audited (service order). Each regression controls for lottery fixed effects, area-of-fund fixed effects, CGU's population category, party fixed effects, municipal characteristics, and interactions of elected mayor and runner-up education attainments to allow for heterogeneous differences according to levels within the non-college group. Standard errors clustered by inspection round in parentheses. ***, ** and * represent $p < 1\%$, $p < 5\%$ and $p < 10\%$ respectively.

Table 4: Restricted sample - potential heterogeneity

(A) <i>First term</i>	$\mathbb{I}(\text{Formal})$ (1)	$\mathbb{I}(\text{Moderate})$ (2)	$\mathbb{I}(\text{Severe})$ (3)	$\mathbb{I}(\text{Any})$ (4)	$\ln(\text{Formal})$ (5)	$\ln(\text{Moderate})$ (6)	$\ln(\text{Severe})$ (7)	$\ln(\text{Total})$ (8)
Estimate	-0.023 (0.057)	0.012 (0.029)	-0.009 (0.059)	-0.051** (0.024)	0.002 (0.024)	-0.093* (0.049)	-0.015 (0.047)	-0.065 (0.042)
N_l	173	173	173	173	173	173	173	173
N_r	165	165	165	165	165	165	165	165
Obs	338	338	338	338	338	338	338	338
(B) <i>No prev. exp.</i>	$\mathbb{I}(\text{Formal})$ (1)	$\mathbb{I}(\text{Moderate})$ (2)	$\mathbb{I}(\text{Severe})$ (3)	$\mathbb{I}(\text{Any})$ (4)	$\ln(\text{Formal})$ (5)	$\ln(\text{Moderate})$ (6)	$\ln(\text{Severe})$ (7)	$\ln(\text{Total})$ (8)
Estimate	-0.048 (0.052)	-0.012 (0.023)	-0.008 (0.058)	0.023 (0.021)	-0.006 (0.022)	-0.125*** (0.045)	-0.012 (0.040)	-0.097** (0.040)
N_l	218	218	218	218	218	218	218	218
N_r	212	212	212	212	212	212	212	212
Obs	430	430	430	430	430	430	430	430
(C) <i>Males</i>	$\mathbb{I}(\text{Formal})$ (1)	$\mathbb{I}(\text{Moderate})$ (2)	$\mathbb{I}(\text{Severe})$ (3)	$\mathbb{I}(\text{Any})$ (4)	$\ln(\text{Formal})$ (5)	$\ln(\text{Moderate})$ (6)	$\ln(\text{Severe})$ (7)	$\ln(\text{Total})$ (8)
Estimate	-0.043 (0.046)	-0.025 (0.021)	0.022 (0.058)	0.006 (0.017)	-0.009 (0.019)	-0.098** (0.046)	0.007 (0.040)	-0.076* (0.044)
N_l	248	248	248	248	248	248	248	248
N_r	231	231	231	231	231	231	231	231
Obs	479	479	479	479	479	479	479	479
(D) <i>PSDB=0</i>	$\mathbb{I}(\text{Formal})$ (1)	$\mathbb{I}(\text{Moderate})$ (2)	$\mathbb{I}(\text{Severe})$ (3)	$\mathbb{I}(\text{Any})$ (4)	$\ln(\text{Formal})$ (5)	$\ln(\text{Moderate})$ (6)	$\ln(\text{Severe})$ (7)	$\ln(\text{Total})$ (8)
Estimate	-0.009 (0.041)	-0.027 (0.0201)	0.025 (0.060)	0.006 (0.017)	0.019 (0.019)	-0.108** (0.050)	0.016 (0.044)	-0.072 (0.048)
N_l	236	236	236	236	236	236	236	236
N_r	223	223	223	223	223	223	223	223
Obs	459	459	459	459	459	459	459	459

Note: This table reports the effect of electing a mayor with college degree on outcomes of mismanagement of public funds while restricting the sample in table 3 to exclude possible drivers. The dependent variable in columns 1-4 is an indicator equals 1 if at least one irregularity in a given severity-category was found (or at least one irregularities in a category, in the case of Any charge). In columns 5-8 the dependent variable is the ln of the number of irregularities in a given severity-category per issue audited (service order). Each regression controls for lottery fixed effects, area-of-fund fixed effects, CGU's population category, party fixed effects, municipal characteristics, and interactions of elected mayor and runner-up education attainments to allow for heterogeneous differences according to levels within the non-college group. Standard errors clustered by inspection round in parentheses. ***, **, * and * represent $p < 1\%$, $p < 5\%$ and $p < 10\%$ respectively.

Table 5: PNAE recommendations: misuse and proportion of misused funds

	Dummy		Share	
	Non-parametric (1)	Parametric (2)	Non-parametric (3)	Parametric (4)
College	0.022 (0.094)	0.018 (0.104)	0.003 (0.023)	0.016 (0.028)
MV	-	-0.248 (0.417)	-	0.107 (0.143)
MV \times college	-	-0.115 (0.472)	-	0.043 (0.148)
N_l	63	63	63	63
N_r	67	67	67	67
Observations	130	130	130	130

Note: This table reports the effect of electing a mayor with college degree on the relative amount of funds misused based on the PNAE recommendation report, which comprises lotteries 34-40. The dependent variable is the surplus amount pointed by CGU auditors that exceeded market-value expenses or for which there was no documentation attesting the correct use of the fund. Standard errors clustered by inspection round in parentheses. ***, ** and * represent $p < 1\%$, $p < 5\%$ and $p < 10\%$ respectively.

Table 6: Reduced form effect of electing a college mayor in term t on future Inter-government transfers in $t + 1$.

	Current election-mandate			Subsequent election-mandate		
	Overall (1)	Federal (2)	State (3)	Overall (4)	Federal (5)	State (6)
Estimate	0.093	0.030	0.027	0.666***	0.895***	0.596**
-	(0.231)	(0.283)	(0.235)	(0.218)	(0.282)	(0.237)
N_l	180	180	180	180	180	180
N_r	163	163	163	163	163	163
Obs	343	343	343	343	343	343

Note: This table reports the effect of electing a mayor with college degree on receiving transfers from federal government and from state government. The dependent variables are the ln of the given transfer category per capita. Columns 1-4 refer to transfers received during the current election-mandate (i.e. the audited mandate), while columns 5-8 refer to the following mandate. *Overall Intergov* stands for the total amount of intergovernmental transfers received during the mandate, while the other outcomes restricts the dependent variable only to discretionary transfers. Each regression controls for lottery fixed effects, area-of-fund fixed effects, CGU's population category, interaction of elected mayor education attainment and runner-up, party fixed effects, and municipal characteristics. Standard errors clustered by inspection round in parentheses. ***, ** and * represent $p < 1\%$, $p < 5\%$ and $p < 10\%$ respectively.

Table 7: Alternative measures: Administrative expenses, $\Delta\%$ Tenured public staff, $\Delta\%$ Temporary staff, and Reelection prospects

(A) Overall	Administrative (1)	$\Delta\%$ Tenured (2)	$\Delta\%$ Temporary (3)	Rerun (4)	Reelected (5)
Estimate	-0.076*** (0.027)	-0.033* (0.019)	-0.368* (0.199)	-0.015 (0.063)	0.013 (0.056)
N_l	262	264	261	264	264
N_r	256	255	250	256	256
Obs	518	519	511	520	520
(B) First term	Administrative (1)	$\Delta\%$ Tenured (2)	$\Delta\%$ Temporary (3)	Rerun (4)	Reelected (5)
Estimate	-0.048 (0.032)	-0.018 (0.024)	-0.389* (0.232)	0.001 (0.080)	0.020 (0.084)
N_l	173	173	171	173	173
N_r	165	164	160	165	165
Obs	338	337	331	338	338
(C) No prev. exp.	Administrative (1)	$\Delta\%$ Tenured (2)	$\Delta\%$ Temporary (3)	Rerun (4)	Reelected (5)
Estimate	-0.080*** (0.022)	-0.017 (0.030)	-0.403* (0.216)	-0.035 (0.057)	0.005 (0.073)
N_l	217	218	215	218	218
N_r	212	211	206	212	212
Obs	429	429	421	430	430
(D) Males	Administrative (1)	$\Delta\%$ Tenured (2)	$\Delta\%$ Temporary (3)	Rerun (4)	Reelected (5)
Estimate	-0.083** (0.040)	-0.038** (0.018)	-0.328* (0.174)	-0.046 (0.074)	-0.001 (0.058)
N_l	246	248	245	248	248
N_r	231	230	225	231	231
Obs	477	478	470	479	479
(E) PSDB=0	Administrative (1)	$\Delta\%$ Tenured (2)	$\Delta\%$ Temporary (3)	Rerun (4)	Reelected (5)
Estimate	-0.053 (0.038)	-0.001 (0.016)	-0.200 (0.155)	-0.044 (0.092)	0.005 (0.067)
N_l	234	236	234	236	236
N_r	223	222	218	223	223
Obs	457	458	452	459	459

Note: This table reports the effect of electing a mayor with college degree on the ln of Administrative expenses as a share of total municipal expenses, % Variation of tenured administrative staff between the first year and last year of the mandate, and % Variation of temporary staff (cargos comissionados) between the first year and last year of the mandate, along with results on the likelihood to rerun in the subsequent municipal election and to get reelected. Each regression controls for term fixed effects, population, interaction of mayor's education attainment and runner-up's, party fixed effects, and municipal characteristics. Standard errors clustered by term in parentheses. ***, ** and * represent $p < 1\%$, $p < 5\%$ and $p < 10\%$ respectively.

A Appendix

Table A1: Level - number of irregularities per issue audited

(A) <i>Linear</i>	Formal (1)	Moderate (2)	Severe (3)	Total (4)
Estimate	-0.0102 (0.026)	-0.4089*** (0.153)	0.0280 (0.069)	-0.3911** (0.174)
Baseline mean	0.2047	2.594	0.4513	3.25
N _l	264	264	264	264
N _r	256	256	256	256
Obs	520	520	520	520
(B) <i>Quadratic</i>	Formal (1)	Moderate (2)	Severe (3)	Total (4)
Estimate	0.0217 (0.042)	-0.5376*** (0.200)	0.0344 (0.098)	-0.4815** (0.229)
Baseline	0.2047	2.594	0.4513	3.25
N _l	264	264	264	264
N _r	256	256	256	256
Obs	520	520	520	520

Note: This table reports the effect of electing a mayor with college degree on outcomes of mismanagement of public funds. The dependent variable in columns 1-4 is the number of irregularities in a given severity-category per issue audited (service order). Standard errors clustered by inspection round in parentheses. ***, ** and * represent p<1%, p<5% and p<10% respectively.

Table A2: Placebo - College candidate vs college candidate

(A) <i>Linear</i>	$\mathbb{I}(\text{Formal})$ (1)	$\mathbb{I}(\text{Moderate})$ (2)	$\mathbb{I}(\text{Severe})$ (3)	$\mathbb{I}(\text{Any})$ (4)	$\ln(\text{Formal})$ (5)	$\ln(\text{Moderate})$ (6)	$\ln(\text{Severe})$ (7)	$\ln(\text{Total})$ (8)
Estimate	0.0273 (0.082)	0.0427 (0.038)	-0.0122 (0.070)	0.0091 (0.032)	0.0018 (0.041)	0.0286 (0.077)	-0.0154 (0.055)	0.0262 (0.078)
N_I	163	163	163	163	163	163	163	163
N_r	163	163	163	163	163	163	163	163
Observations	326	326	326	326	326	326	326	326
(B) <i>Quadratic</i>	$\mathbb{I}(\text{Formal})$ (1)	$\mathbb{I}(\text{Moderate})$ (2)	$\mathbb{I}(\text{Severe})$ (3)	$\mathbb{I}(\text{Any})$ (4)	$\ln(\text{Formal})$ (5)	$\ln(\text{Moderate})$ (6)	$\ln(\text{Severe})$ (7)	$\ln(\text{Total})$ (8)
Estimate	0.0769 (0.111)	0.0632 (0.051)	-0.0027 (0.107)	0.0374 (0.043)	0.0396 (0.051)	-0.0143 (0.091)	-0.0145 (0.080)	0.0169 (0.098)
N_I	163	163	163	163	163	163	163	163
N_r	163	163	163	163	163	163	163	163
Observations	326	326	326	326	326	326	326	326

Note: This table reports results of a placebo exercise using elections in which both candidates have a college degree. For this exercise, I split this sample evenly and assign randomly a placebo college-treatment. The dependent variable in columns 1-4 is an indicator equals 1 if at least one irregularity in a given severity-category was found (or at least one irregularity of any category, in the case of Any charge). In columns 5-8 the dependent variable is the \ln of the number of irregularities in a given severity-category per issue audited (service order). . Standard errors clustered by inspection round in parentheses. ***, **, and * represent $p < 1\%$, $p < 5\%$ and $p < 10\%$ respectively.

THIRD CHAPTER
ACCOUNTABILITY SHOCK AND MARKET FOR
OVERSIGHT

3.1 Introduction

In public governance, transparency is arguably the most important feature in spurring lasting accountability, therefore inducing sustainable pro-social behavior within public institutions (Khemani et al., 2016). It is claimed that the main reason for such is that citizens are inherently motivated-agents, who benefit from public goods and services that would otherwise be compromised by extraction of political rents - or simply by inefficient management. Although this reasoning sounds obvious, the circumstances under which this mechanism holds are not: The literature doesn't fall short on cases in which citizens simply don't react or even refrain from political engagement after being exposed to new information (for instance, see Chong et al., 2015).

In this paper I test whether an exogenous top-down shock in oversight and political accountability can strengthen the market for transparency in public management, focusing on non-electoral mechanisms, such as citizens' demand for information, civil complaints and denouncements, and legal accountability. As the source of this accountability saliency, I take advantage of a Federal anti-corruption program in Brazil, which selects municipalities through publicly held lotteries for inspection of proper use of federal transfers and then discloses the findings to the general public, which has already been shown to have media repercussion (both data evidence and anecdotal examples on media coverage of the released reports appear in Ferraz and Finan, 2008, 2011; Litschig and Zamboni, 2016; Muço, 2017, just to cite a few).

Objectively, I analyze three blocks of outcomes in a panel-data fashion. The first comprises measures of citizens' engagement as measured by: demand for information on public management through official requests, based on the Federal Law of Information Access, and citizens' interactions with the Federal ombudsman for complaints and denouncements regarding public management. It is crucial that both aforementioned measures are not susceptible to strategic behavior by municipal administration, as I discuss in section 3.3. The second block regards outcomes of the judicial system, which includes the number of lawsuits filled against public agents, including lawsuits

filled by common citizens. At last, I analyze measures of local governments' supply of transparency and institutional demand for oversight, which consider the likelihood of municipalities adopting a local ombudsman and partnering with the federal government to implement best practices in public transparency.

Results show that disclosure of information on the local use of public funds, on average, increases citizens' demand for information on public affairs to the Federal government through the Federal Law of Information Access. On the side of active supply of oversight, I also show that disclosure boosts citizens' interaction with the Federal ombudsman, which is in part driven by formal denouncements and complaints against public officials and institutions. On the judicial side, disclosure is followed by an increase in the number of lawsuits filed against public agents both by public prosecutors (i.e. Public Prosecutor General) and by ordinary citizens. At last, I show that, in municipalities exposed to information disclosure, only the exposed political institution displays an increase in compliance with transparency guidelines, whereas a zero effect exist for institutions not directly exposed.

The literature on social over-watch of public institutions is mostly unable to estimate any causal effect of citizens' demand for transparency (thereof supply of oversight) because of the simultaneous relation between the quality of institutions and the citizens' level of interest/effort in enforcing accountability. On the other hand, the existing causal evidence supporting the accountability role of private individual agents in punishing corruption focus on the channel of agents as being voters who, in the presence of negative information, punish public agents through elections, thus neglecting the view of citizens as active over-watchers of the public machine.

The remainder of this paper is structured as follows: Section 3.2 provides a picture of Brazilian local administration and of the program exploited for disclosure of information on the use of public funds. Section 3.3 discusses the outcomes analyzed, their data-sources, and displays summary statistics. Section 3.4 discusses the empirical strategy used to provide the results and that to follow in the later stages of this paper. Finally section 3.5 presents the findings and discusses results, while further remarks

and plans on how to develop this project are proposed in section 3.6.

3.2 Institutional background

Brazilian municipalities enjoy the status of federation members, not being subordinated to the State or Federal governments. This strong decentralization lends municipal administrations, in the figure of the local executive office, to be the main actors in local expenditures, policy implementation, and provision of basic health care, primary education, and sanitation infrastructure. Nevertheless, Brazil's tributary structure concentrates taxes collection at the Federal and State levels, with municipal taxes representing only 6% of total municipal revenue (Brollo and Troiano, 2016). To put it into perspective, a third of Brazilian municipalities are not even able to generate enough revenue to pay the salary of its mayors¹

Following this centralized tax collection by Federal and State executive administrations, revenue is then distributed across municipal administrations over constitutional transfers and discretionary transfers. Most of those transfers are earmarked, meaning they are intended by law to be invested in a specific area of public good and services provision (e.g., mainly health, education, and sanitation).²

In response to the lack of state capacity to detect malfeasance in this highly decentralized administrative structure, Controladoria Geral da União (CGU), a ministry that centralizes the government's internal control and sets directives for promoting efficient use of public resources, launched a program consisting of randomly assigned audits of municipalities for their proper use of federal transfers. Since the latter represents the major part of municipal budgets, such program became a milestone tool to deter misuse of public funds in Brazil (Avis, Ferraz and Finan, 2017).

In each round of the program, which has audited more than R\$22 billion worth of

¹ O Estado de S.Paulo "Um terço dos municípios do País não gera receita nem para pagar salário do prefeito" <https://economia.estadao.com.br/noticias/geral,um-terco-dos-municipios-do-pais-nao-gera-receita-nem-para-pagar-salario-do-prefeito,70002473456> (Last accessed on February 05 2019)

² As will be discussed in depth in section 3.3, the centralized nature of tax collection - which feeds municipal budgets with revenue to be invested in specific areas - make it reasonable for citizens to requests information and fill complaints over local issues directly to the federal government.

federal transfers, municipalities are randomly chosen for inspection through publicly held lotteries. Once municipalities are drafted, CGU sends a team of auditors to cross-check accounts and documents, inspect the delivery of public services, and verify the existence and quality of infrastructure to which the federal transfers were intended. Shortly after the information from field inspections are reported to CGU's central office, detailed reports with the results for each inspected municipality are published on-line and sent to the competent authorities, including federal courts and the local judiciary, with visible repercussion in the media and to federal, state, and local authorities (refer to Ferraz and Finan, 2008, 2011; Litschig and Zamboni, 2016; Avis, Ferraz and Finan, 2017; Muço, 2017, for specific examples).³

From 2006, on the 20th round of the program, the audit reports started to be compiled internally by CGU into readable data, containing the dates of the lotteries and report release, the area to which the fund should have been invested, the government program to which the transfer was attached, the amount transferred, and the quantity of irregularities found classified as formal/procedural, moderate, or severe, according to degree of materiality and compromise to the program. In later versions of this paper, as will be further discussed in section 3.4, I will use the number of irregularities as well as their severity to try and decompose this effect of exposure between a simple change in the perceived relevance of the topic after it is suddenly brought up (i.e. exposure vs non-exposure) and eventual heterogeneity according to the intensity of public funds misuse.

3.3 Data and descriptive statistics

Below I describe the outcomes from each sub-block mentioned in the introduction along with summary statistics. Table 1 displays it for the overall sample as well as disaggregated by treatment group.

³ Reports can be accessed and downloaded through <http://auditoria.cgu.gov.br/>, under *Fiscalização em Entes Federativos - Municípios > Fiscalização Sorteio de Municípios*

Demand for information For the outcomes on citizen's request for information on public affairs, data were obtained from eSIC, the Electronic Information System to Citizens, from the Brazilian Federal Government. eSIC is an online platform through which citizens can exercise their right to access of information, introduced by the Law of Access to Information (LAI - law number 12.527, passed in 2011).

The outcomes are defined as follows, while descriptive statistics for these variables are presented in panel A of table 1: *# Requests* refers to the total number of requests for information/data on public affairs coming from a municipality; *Natural person* regards the number of requests whose requester is an individual (Pessoa física) in contrast with an institution; *Local services* refers to the number of requests regarding Health, Education or Sanitation, which are the main attributions of the local government, as discussed in section 3.2.

Furthermore, in an attempt to rule out that the number of requests is being driven by specific groups, such as journalists gathering input for writing news or politicians gathering information, for instance, to produce bad campaigning against adversaries, I disaggregate the number of requests into the following subgroups: *Journalist* refers to requests whose requesters are self-declared journalists; *Public servant* regards the number of requests whose requesters are self-declared public servants; *Politician* concerns the number of requests whose requesters are self-declared politicians; and *Complement* is simply the number of requests whose requesters are natural person (Pessoa física) excluding those belonging to the aforementioned categories.

One last measure of interest is *Days to reply*, which measures the number of days it took for the eSIC system to provide the requester with the first answer. This last variable is important because any eventual difference in the number of days to reply between audited and non-audited municipalities may suggest that treatment effect comes not only from the inspection but also from a supply-side adjustment in the provision of information.

Supply of oversight Data on interactions between citizens and the Federal Ombudsman were obtained from eOuv, The Electronic Ombudsman system, from the Brazilian

Federal Government. eOuv is an online platform through which citizens can fill formal and anonymous denouncements and complaints against public-service providers or individuals public agents.

The outcomes are defined as follows, while descriptive statistics for these variables are presented in panel B of table 1: # *Contacts* refers the total number of citizen-to-federal-ombudsman contacts, coming from a municipality; *Denouncement* regards the interactions, from the total number of contacts, algorithmically classified by the eOuv system as being a denouncement; *Complaints* refers to the number of contacts algorithmically classified by the eOuv system as a complaint; while *Compliments*, for which I expect either a negative relation with being audited or no difference at all, is the number of contacts algorithmically classified by the eOuv system as a compliments. Following the same reasoning as for eSIC, *Days to reply* measures the number of days it took for the system to provide the citizen with the first answer, for which there should be no difference between the two groups.

Rule of law Data on judicial outcomes were provided by a third party, being scraped directly from courts. The outcomes are defined as follows, while descriptive statistics for these variables are presented in panel C of table 1. # *Lawsuits*: refers to the total number of lawsuits filled against public agents in the municipality; # *Defendants/lawsuit*: regards the average number of defendants represented per lawsuit; # *Issues/lawsuit*: concerns the average number of issues (“violations”) presented per lawsuit; *Prosecutor General*: is a dummy for lawsuits whose plaintiff is the Public Prosecutor General (Ministério Público); and *Natural person*: is the number of lawsuits whose plaintiff is an individual (Pessoa física) in contrast with an institution.

Local supply of transparency As the first measure of local compliance with transparency guidelines I use the indicator of adoption of a local eOuv by the municipal institutions. This program aims at spreading the adoption of an ombudsman along the lines of the federal eOuv at local institutions, having reached signatory entities across 366 municipalities by May 2018. It provides the source code for the municipality-

adapted version of e-Ouv free of charge, with no need for the institution to install the system locally, along with technical support and training.

The program works upon voluntary adoption at the institutional level (in contrast with the municipal level), therefore the variables *Prefecture* whether the mayor cabinet signed the adoption term of the program and *Legislative* whether the local legislative chamber signed the adoption term of the program. An important remark is that the municipal executive office, in the figure of the Prefecture, is the ultimate responsible for implementing policies and incurring expenses, thus being the one to blame in case of misuse of funds. For this reason, the Prefecture is considered the exposed institution once the reports are disclosed, while the legislative chamber is taken as a comparative group. Descriptive statistics for these variables are presented in panel D of table 1.

Transparent Brazil For the second measure of local compliance with transparency guidelines I use an indicator of whether municipal institutions signed a commitment term to improve their access to information according to the LAI - law number 12.527. Up to December 2017, 1,566 municipalities had at least one signatory institution.

This program aims at improving transparency in public management by providing public servants with training on the LAI and how to comply with it. The training also comprises how to structure and publish open data on local public affairs and on how to install a local eSIC following the standards of the Federal eSIC. The program works upon voluntary adoption at the institutional level (in contrast with the municipal level), therefore the variable *Prefecture* indicates whether the mayor cabinet signed the commitment term whereas *Legislative* indicates whether the local legislative chamber signed it. The same reasoning for the local eOuv applies here; since the Prefecture is the one to blame in case of misuse of funds, it is considered as the institution exposed by the report, while the legislative chamber is taken as a comparative group. Descriptive statistics for these variables are presented in panel E of table 1.

[Table 1 about here.]

3.4 Empirical strategy

The empirical strategy adopted is a difference-in-differences with an exogenously assigned treatment, where i denotes the municipality and t denotes the quarter. The current specification is as follows in equation 1, where α_i is the municipal fixed effect, λ_t is the quarter fixed effect, and $published_{it}$ is a dummy that assumes value 1 after the report is published in the audited municipality municipality i . This implies that β is the first causal effect of interest, measuring the average effect of disclosure of information regardless of the type of information released (i.e irrespective to the intensity of misuse reported)

$$(1) \quad y_{it} = \beta \text{published}_{it} + \alpha_i + \lambda_t + \varepsilon_{it}$$

In later versions, however, I consider using the specification shown in 2, which will absorb the effect of the report release while taking in consideration the intensity of the misuse reported, whose effect is measured by β_1 . Moreover, X'_{it} is a vector of time-variant controls also to be included.

$$(2) \quad y_{it} = \beta_1(\text{published}_{it} \times \text{infringements}_i) + \beta_2 \text{published}_{it} + \alpha_i + \lambda_t + X'_{it}\delta + \varepsilon_{it}$$

3.5 Results

In this section I present results to support the hypothesis raised thus far. In the first step, results point that citizen's interest in public management increase after a leveling in the otherwise asymmetric information on the quality of management. Those results are in line with anecdotal evidence of citizens denouncements (named by CGU as denúncia cidadã or citizen denouncement), which lead to legal investigations that held corrupt agents accountable and to face prosecution, as is the case for instance of Operation Xequê Mate, in which a citizen complaint presented to CGU lead to investigations regarding the misuse of earmarked transfers in the amount of R\$ 424,695

(or about US\$ 105,150).⁴

Demand for transparency After the release of reports, audited municipalities show an increase of 7.448% in the total number of requests, of which more than 50% are from individual citizens - in contrast with institutions. Notice that most of this increase is driven by requests regarding education, health or sanitation. Moreover, results in columns (5)-(7) in table 2 rule out that media, politicians, and bureaucrats are the responsables for this increase in demand of transparency.

At last, the estimate for days to reply, in column (8), is virtually zero both statistically and in absolute value when confronted to the sample mean, which is 15.401 days. This is a comforting results as it supports that this increase in demand for public information isn't being driven by an increase in Federal efficiency targeted to audited municipalities.

[Table 2 about here.]

Supply of oversight In table 3 we see that just as in the case of eSIC, the release of reports causes a 50% increase in the total number of contacts to the federal ombudsman, which is mostly driven by denouncements and complaints. For compliments, which is reasonable to expect either a negative relation with being audited or no difference at all, we see it is the latter. Moreover, the estimate for days to reply, in column (5), is also virtually zero both statistically and in absolute value., which reinforces that this increase in supply of oversight isn't being driven by an increase in Federal efficiency targeted to audited municipalities.

[Table 3 about here.]

Legal source of accountability? Table 4 shows, as expected, an increase in the number of filled lawsuits against public officials following the release of audit reports -

⁴Not only operations like this make responsables accountable, they also prevent further misuse. In the case operation Xequê Mate, the accused institution was in the list to receive other additional R\$ 4 mil (or about US\$ 989,694). <http://ouvidorias.gov.br/raio-ouvidorizador/cgu-e-pf-desarticulam-desvios-praticados-por-associacao-de-enxadristas-em-rondonia> (Last access on February 12 2019)

column (1). It also shows that the number of defendants indicted per lawsuit increases - column (2) - as well as the number of different accusations within the same lawsuit - column (3). This phenomenon is expected even in the absence of citizens' oversight, since public institutions such as the Public Prosecutor General (hereafter PG) would likely fill suits against those involved in the newly found malfeasance anyway.

Although the PG usually acts by itself in defense of society best interest, it is also responsible to intervene upon denouncements made directly by ordinary citizens, since the latter are less likely to pursue legal accountability against public officials if they have to incur all the costs of doing so by themselves.⁵

Because it is not possible to disentangle lawsuits filled by PG acting upon denouncements made directly by ordinary citizens from those in which the PG would file a suit even in the absence of citizens' denouncements, in column (4) I display the estimate for all suits in which the PG is a plaintiff. This accounts for barely two-thirds of the increase in the total number of filled lawsuits, as expected. On the other hand, column (5) displays the estimate for all suits in which at least one direct plaintiff is a natural person (in contrast with an institution). Although it is a noisy measure, since it doesn't account for cases in which the PG acting upon citizens denouncements but the citizen is not a plaintiff herself, I still find a positive estimate.

At last, column (6) displays an estimate of 4.524 days-difference in the length of judicial cases between audited and non-audited jurisdictions. This is not only statistically insignificant, but also economically, when confronted to the average length of 376 days. Such result is analogous to that of days to reply in the case of eSIC and eOuv. This suggests to some extent that the efficiency of the judicial system is not being affected by the report disclosure itself.

[Table 4 about here.]

Supply of Transparency Table 5 shows that audited municipalities present an improvement in transparency measures and in effort to support transparency, such as

⁵ Every Brazilian citizen is entitled to propose a popular judicial action (ação popular, which is legislated by law 4.717, 1965) against abusive behavior committed by public agents.

adopting a local ombudsman and partnering with the federal government to implement best practices in public transparency.

An important remark is that the municipal executive office, i.e the Prefecture, is the ultimate responsible for implementing policies and incurring expenses using the transfer received from higher levels of government. Therefore, the prefecture is the main institution exposed by the reports, as it is the one to blame in case of misuse of funds. The legislative is not entitled to incur costs with funds from those transfers, as well as not being involved in bidding processes, where infringements such as over-invoicing occur. This said, it is interesting that after disclosure of information, prefectures become more likely to sign the commitment term while local chambers are inert.

[Table 5 about here.]

Given the simultaneity between supply and demand when observing market equilibrium, the results presented in table 5 support that local institutions are shifting their supply curve. This would reveal a higher demand-level for transparency at the municipal level even in the absence of any structural increase in citizens' engagement. This prevents us from interpreting the results shown for eSIC and eOuv as an approximation to the magnitude of demand for public information and supply of oversight at the local level.

3.6 Final remarks

In sum, results suggest that, on average, disclosure of information on misuse of public funds (i) increases citizens' demand for information on public affairs, (ii) boosts citizens interaction with the Federal ombudsman, also increasing the number of denunciations and complaints against public officials and institutions, (iii) increases the number of lawsuits filled against public agents both by public prosecutors and by ordinary citizens, and (iv) promotes compliance with transparency guidelines by the exposed institutions. Those results, nevertheless, need additional scrutiny, and

a brief research plan for further developments is describe as follows: To disentangle the effect of saliency of simply bringing the topic of proper use of public funds from the effect of release of information on misuse of public funds, I need to include the number of infringements in regression. Moreover, supposing that those phenomena happen through consumption of information, it is reasonable to expect the media market to have a strong role in popularizing the information found in those reports. Therefore, measures of internet penetration as well as radio and tv broadcasts in the municipalities is needed.

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Table 1: Summary statistics - Information requests (eSIC) & Federal Ombudsman (eOUV), Legal accountability, Adoption of ombudsman, Transparent Brazil

Variables	Overall		audited=0		audited=1	
	mean	sd	mean	sd	mean	sd
<i>(A) Information requests (eSIC)</i>						
# Requests	3.270	8.416	3.101	8.609	4.382	6.918
Natural person	3.079	8.317	2.934	8.551	4.039	6.495
Local services	1.167	3.414	1.094	3.370	1.647	3.654
Complement	2.184	4.877	2.059	4.760	3.003	5.516
Journalist	0.041	1.312	0.044	1.405	0.025	0.228
Politician	0.017	0.300	0.018	0.315	0.014	0.173
Public servant	0.838	6.443	0.813	6.820	0.998	2.924
Days to reply	14.35	34.20	13.97	31.64	16.84	47.68
Municipalities	3,499		3,100		399	
<i>(B) Federal Ombudsman (eOUV)</i>						
# Contacts	2.198	4.879	2.047	3.081	3.692	12.73
Denouncement	0.810	1.750	0.751	1.231	1.390	4.235
Complaints	0.659	2.717	0.605	2.043	1.192	6.213
Compliments	0.027	0.279	0.024	0.279	0.052	0.287
Days to reply	21.27	48.12	21.20	48.42	22.03	44.99
Municipalities	3,131		2,891		240	
<i>(C) Legal accountability</i>						
#Lawsuits	3.555	7.670	3.336	7.798	4.665	6.882
#Defendants/lawsuit	9.355	23.48	8.758	23.50	12.38	23.12
#Issues/lawsuit	9.842	36.37	9.052	38.19	13.84	24.92
Prosecutor	2.307	5.518	2.168	5.604	3.012	5.008
Natural person	0.428	3.738	0.419	4.051	0.472	1.279
Municipalities	1,388		1,204		184	
<i>(D) Adoption of ombudsman</i>						
Prefecture	0.023	0.149	0.022	0.146	0.056	0.229
Legislative	0.002	0.044	0.002	0.042	0.007	0.086
Municipalities	3,577		3,487		90	
<i>(E) Transparent Brazil</i>						
Prefecture	0.165	0.371	0.158	0.365	0.216	0.412
Legislative	0.017	0.127	0.016	0.124	0.023	0.150
Municipalities	3,936		3,487		449	

Table 2: Difference-in-differences estimates: Effect of Audits release on requests for information through eSIC

	# Requests	Natural person	Local services	Complement 5-7	Journalist	Politician	Public servant	Days to reply
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Published</i>	0.503** (0.221)	0.489** (0.229)	0.250** (0.127)	0.349* (0.188)	0.006 (0.020)	0.039 (0.042)	0.095 (0.092)	0.411 (1.304)
Municipality F.E	YES	YES	YES	YES	YES	YES	YES	YES
Quarter F.E	YES	YES	YES	YES	YES	YES	YES	YES
State trend	YES	YES	YES	YES	YES	YES	YES	YES
R-squared	0.720	0.705	0.597	0.711	0.214	0.344	0.353	0.163
% Δ from Baseline	7.448	7.386	5.951	5.939	0.154	0.511	2.605	2.668
Municipalities	3,536	3,536	3,536	3,536	3,536	3,536	3,536	3,536

Note: This table presents difference-in-differences estimates of the effect of Audits release on requests for information on public management to the Federal Government, through eSIC *Sistema Eletronico do Servico de Informacao ao Cidadao*. # *Requests* is the total number of requests coming from a municipality; *Natural person* denotes an individual (represented by a social security number) in contrast with an institution; *Local services* denotes to requests regarding Health, Education or Sanitation, which are the main attributions of the local government; *Complement 5-7* refers to all requests excluding those coming from the classes considered in columns (5) to (7); *Journalist*, *Politician*, and *Public servant* denote requests whose requesters exerted each of those functions, respectively; *Days to reply* denotes the numbers of days it took between the request was made and the first reply regarding the information requested. % Δ from Baseline is the relative variation of $\hat{\beta}_{DID}$ with respect to the outcomes' mean value for the control group. Specifications include municipality fixed effects, Quarter fixed effects. Standard errors clustered at the municipal level are presented in parentheses. ***, ** and * represent $p < 1\%$, $p < 5\%$ and $p < 10\%$ respectively.

Table 3: Difference-in-differences estimates: Effect of Audits release on Citizens' interactions with the Federal Ombudsman, through eOUV.

Full sample	# Contacts	Denouncement	Complaints	Compliments	Days to reply
	(1)	(2)	(3)	(4)	(5)
<i>Published</i>	1.560** (0.723)	0.388* (0.210)	0.732** (0.341)	-0.002 (0.017)	0.023 (0.981)
Municipality F.E	YES	YES	YES	YES	YES
Quarter F.E	YES	YES	YES	YES	YES
State trend	YES	YES	YES	YES	YES
R-squared	0.453	0.369	0.403	0.203	0.140
Mean Baseline	3.5813	1.9021	3.1837	1.6331	23.3046
%Δ from Baseline	43.565	20.419	22.976	-0.110	0.099
Municipalities	3,149	3,149	3,149	3,149	3,149

Note: This table presents difference-in-differences estimates of the effect of Audits release on Citizens' interactions with the Federal Ombudsman, through eOUV. # *Contacts* is the total number of contacts coming from a municipality; *Denouncement*, *Complaints*, and *Compliments* denote the class of contact; *Days to reply* denotes the numbers of days it took for the Ombudsman office to reply the citizen regarding the deference of the issue reported. %Δ from Baseline is the relative variation of $\hat{\beta}_{DID}$ with respect to the outcomes' mean value for the control group. Specifications include municipality fixed effects, Quarter fixed effects. Standard errors clustered at the municipal level are presented in parentheses. ***, ** and * represent $p < 1\%$, $p < 5\%$ and $p < 10\%$ respectively.

Table 4: Difference-in-differences estimates: Effect of Audits release on lawsuits against public officials.

	# Lawsuits	# Defendants/suit	# Issues/suit	Prosecutor General	Natural person
	(1)	(2)	(3)	(4)	(5)
<i>Published</i>	0.675** (0.311)	2.016** (0.896)	2.487** (1.182)	0.429** (0.168)	0.076* (0.044)
Municipality F.E	YES	YES	YES	YES	YES
Quarter F.E	YES	YES	YES	YES	YES
R-squared	0.345	0.295	0.316	0.304	0.207
%Δ from Baseline	23.818	25.487	34.513	17.361	4.073
Municipalities	1,351	1,351	1,351	1,351	1,351

Note: This table presents difference-in-differences estimates of the effect of Audits release on lawsuits against public officials. # *Lawsuits* refers to the number of lawsuits classified as administrative improbity filled against public officials, # *Defendants/suit* refers to the number of defendants accused in a given lawsuit, # *Issues/suit* refers to the number of law-infringement accusations within the same suit, *Prosecutor General* refers to lawsuits for which the plaintiff in the Prosecutor General (*Ministerio Publico*), *Natural person* denotes an individual in contrast with an institution, and *Length* refers to the length in days between the registration of the lawsuit in the judicial system and the date this data was scraped for lawsuits that were not sentenced yet. %Δ from Baseline is the relative variation of $\hat{\beta}_{DID}$ with respect to the outcomes' mean value for the control group. Specifications include municipality fixed effects, Quarter fixed effects. Standard errors clustered at the municipal level are presented in parentheses. ***, ** and * represent $p < 1\%$, $p < 5\%$ and $p < 10\%$ respectively.

Table 5: Difference-in-differences estimates: Effect of Audits release on local supply of transparency.

	Adoption of ombudsman		Transparent Brazil	
	Prefecture	Legislative	Prefecture	Legislative
	(1)	(2)	(3)	(4)
<i>Published</i>	0.042*** (0.013)	-0.001 (0.004)	0.027*** (0.006)	0.002 (0.002)
Municipality F.E	YES	YES	YES	YES
Year F.E	YES	YES	YES	YES
State trend	YES	YES	YES	YES
R-squared	0.842	0.830	0.893	0.894
% Δ from Baseline	4.160	-0.100	2.660	0.020
Municipalities	3,577	3,577	3,936	3,936

Note: This table presents difference-in-differences estimates of the effect of Audits release on measures of supply of transparency by the public institutions as measured by *Adesao eOuv municipios*, and *Adesao Brasil Transparente*. *Adoption of ombudsman* refers to whether the municipality signed a term to implement a local ombudsman eOuv; *Transparent Brasil* refers to whether the municipality signed a term to implement better practices in transparency and adopt a local eSIC; *Prefecture* denotes whether the mayor hall signed the term, *Legislative* denotes whether the local legislative chamber signed the term. % Δ from Baseline is the relative variation of $\hat{\beta}_{DID}$ with respect to the outcomes' mean value for the control group. Specifications include municipality fixed effects, Year fixed effects. Standard errors clustered at the municipal level are presented in parentheses. ***, ** and * represent $p < 1\%$, $p < 5\%$ and $p < 10\%$ respectively.